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Members of all classes are entitled to receive these publications free: for additional copies of the Journal they will be charged at the price of five shillings per part, and of the brochures at the prices stated on the wrappers, varying in proportion to bulk.

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Communications upon papers read at meetings, notices of personal movements, or other matter for which publication is desired in the next issue of the Journal, should be in the hands of the Editor on or before the last day of December, March, June or September as the case may be. Subsequent delivery may be too late for insertion.

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PERSONAL NOTES OF MEMBERS AND SPECIAL NOTICES.

It is suggested that Members of the Institution send information regarding their movements to the Secretary for insertion under this heading.

Mr. H. T. Burls returned from Ecuador in October.

The honour of a baronetcy has been conferred upon Mr. J. T. Cargill.

Mr. J. A. Child has been demobilised, and went to Venezuela in November.

Mr. A. Lloyd Eastlake, who was demobilised from Special Brigade, R.E., in June 1919, has been appointed Chemical Engineer at Vickers, Ltd., M.L. Experimental Company, Joyce Green, Dartford.

Mr. W. H. Fordham has left for Trinidad.

Mr. R. B. Grey went to the South of Spain in October last.

Mr. J. E. Marshall Hall went to South America in September and returned in January.

Sir Henry Alexander Miers, D.Sc., F.R.S., Vice-Chancellor of the University of Manchester, has been appointed by an Order of Council dated the 16th day of October, 1919, to be a member of the Advisory Council to the Committee of the Privy Council for Scientific and Industrial Research.

Mr. Arthur Millar is now in Egypt.

Mr. Launcelot Owen is in Colombia, South America, on exploratory work.

Mr. A. F. Strickland has gone to California to take up some oil engineering work.

EDITORIAL.

Retrospect and Prospect.

At the opening of a new session and a new volume, it is fitting to look back upon the past, and forward to the future, of our career. We can view with no small measure of complacency the six years of our existence, the first, one of practically silent infancy, the other five, of increasing vitality in publication, and in even more potent correspondence. Whilst our five volumes, with more than forty formal papers, and the discussions ensuing upon their presentation, cover a vast range of topics connected with our wide-scoped subject, handled in most comprehensive and masterly style by our experienced contributors, it is probable that even yet more extensive service has been rendered privately, either in the direct form of information, or as suggestion of likely or certain sources whence trustworthy data may be obtained. Such private communications are at least made in quarters where they are needed, whilst matter published in the Journal is as seed scattered on the wind, to germinate or not as chance may befall. There is, however, much fertile soil in our extending membership, and harvest prospects are extremely encouraging.

But however satisfactory may appear, or may have really been, the past career of the Institution, its present, and still more, its immediate future are matters of transcending import.

As we remarked in these pages nearly two years ago, a stationary attitude in a time of general advance is relatively retrogression; we must not think of resting on our oars, and trusting to the current. Realising the past utility of our combination, and its great potentialities for good, so largely kept latent by conditions (physically as well as financially narrow), several of the largest oil Companies have agreed to subsidise the Institution to an extent that enables immediate development in certain minor directions, and highly important activities when the details shall have been decided. An influential Committee has been formed for considering the most practicable methods of extending the functions of the Institution, and a primary step has been taken in securing more suitable accommodation for our rapidly-increasing library, and better conditions for its utilisation by our members. Before this issue is distributed we hope to be in possession of the premises whose address appears on the title-page, and the removal and

rearrangement of the library will be effected with the shortest possible suspension of its accessibility to members. There is yet to be incorporated with the library Sir Boverton Redwood's extensive and valuable collection of scientific books, &c., presented to the Institution by Lady Redwood.

A second point of improvement contemplated is the publication in the JOURNAL, of a brief summary of the more important advances in the sciences and arts connected with petroleum, whether published or communicated *de novo*. The proposed extension of the JOURNAL, some eight or ten pages per issue, would be a compilation by writers conversant with the current development in the several sub-divisions. A separate section would be reserved for communications of recent discoveries, scientific or technical, not elsewhere published, and too limited to constitute the basis of more than a brief note. Such might be the discovery of a new oilfield (not merely outlier or extension of known areas), a new agent or principle of operation in exploiting, refining, transport or utilisation, and the like.

A desideratum in the Library is a bibliography of all that relates to petroleum, kept up to date nearly as may be practicable, and therefore partly at least in MS., (or typed), and all on cards or slips, in order to admit of the daily additions to the literature of our wide-ranging topic effected by the energy of research in all parts of the world. An essential adjunct to this bibliography would be a thorough subject-index. By long and assiduous collection, the writer possesses what he believes to be the most complete bibliography extant, doubtless far short of perfection (some hundreds of entries yet requiring correction or addition) but nevertheless, the nearest approach thereto. This, his private property, is at the service of the Institution for consultation, with such unavoidable delay as is involved in its retention at his residence. Possibly, with the improvement in the environment of the Institution, some mode of duplication of the valuable MS. may be effected. At present there are about 14,000 entries of titles, apart from those of subject-index.

We do not propose on the present occasion to deal with the wider schemes under consideration by the Committee. Some have already been adumbrated in remarks published in the Journal: others are of more recent origination than our last issue.

We enter upon our new session with sanguine anticipation of successful combat with the difficulties that lie in the way of realising our aim—the advance, upon solid and permanent lines, of the whole group of industries based upon petroleum. The good work

effected in the past with very limited funds, by the trustworthy hands guiding the activities of the Institution, will assuredly be greatly augmented by the hearty co-operation of all members, and the financial support and goodwill of the Companies and firms engaged in the various branches of the vast industry which we represent.

REVIEW.

Popular Oil Geology.

By VICTOR ZIEGLER,

Professor of Geology and Mineralogy, Colorado School of Mines. Pp. viii, 149. 16mo, *New York* (John Wiley & Sons), and *London* (Chapman & Hall, Ltd.), 1918.

This little book is an elaboration of a series of lectures delivered in Colorado and Denver during the winter 1917–18, and is itself intended as an introduction to the author's forthcoming "Principles of Oil Geology," and a whet to the appetite for further acquaintance with this important topic.

Few recent books have been issued which treat their subject so comprehensively and yet concisely as this excellent little summary of the leading principles of the geology of petroleum, written, not for the experienced, but for those without technical or scientific training in the subject, so as to be intelligible to the layman interested practically or financially, or simply desirous of extending his knowledge in this direction.

The history of the petroleum industry, the elementary principles of chemistry, the debated origin of oil, and the primary facts of geology constitute in a few pages the foundation of the work, on which arises the compact superstructure of the stratigraphical and other conditions bearing on the accumulation of oil and gas in quantities of commercial importance. The illustrations are naturally drawn chiefly from the author's personal studies in Wyoming and Colorado, but several other States furnish those of exceptional structures.

A short chapter is devoted to oil-shales, those of Colorado being estimated as carrying ten times as much oil as the entire produce of the United States in the 60-years' development of the oil industry.

This most useful work concludes with a brief admonitory chapter on oil investments, quoting (with the requisite substitutions) J. H. Hammond's rules for mining investments, not the less sage for their humorous setting.

Fortieth General Meeting.

This Meeting was held at the House of the Royal Society of Arts, John Street, Adelphi, W.C. 2, on Tuesday, October 21st, 1919, Sir Frederick Black (President) occupying the Chair.

The Chairman said he had great pleasure in welcoming the members at the commencement of a new session. It was very delightful to see members present like their old friend Sir Thomas Holland, who had come from India. Sir Thomas was only home for a short time, but he hoped they would hear him join in the discussion that evening.

It was hardly possible to commence the new session without a few brief words as to the loss the Institution had sustained by the death of Sir Boverton Redwood. The Council of the Institution had sent a letter to Lady Redwood in which they tried to express what they felt, and what they thought the whole of the members of the Institution felt as to Sir Boverton Redwood's distinction in his profession, and above all as to what he was as a man. It had come to him not quite as a surprise, but it was remarkable to note the expressions of opinion from different members of the Council with regard to Sir Boverton's great kindliness of manner and his great human heart, which had appealed to everyone even more perhaps than his great professional attainments and distinction. The Institution owed Sir Boverton a very great deal, far too much to dilate upon. There was no formal resolution to pass upon the subject as the Council had done all that was necessary, but he felt that it was not possible to begin the work of the new session without some reference to one who was almost the founder of the Institution, who was its inspiration, one of its most valued members, and its first, and he thought he ought to say its greatest, President.

He had great pleasure in introducing the lecturer, Mr. Arnold Philip. Somebody had asked him whether he knew Mr. Arnold Philip, and his reply had been "Don't I?" Looking around he saw some of his new colleagues that he had met in the City, and other of his old colleagues that he used to meet in the Government departments. He had had the pleasure of being the sole audience of Mr. Arnold Philip at some of his chemical lectures, and he had had to perform the functions of a great many individuals: he had to try and look interested, which was not difficult, and to try and look intelligent, which was more difficult, and sometimes to put on as a compliment to Mr. Philip's great knowledge of the subject a puzzled look, so that he might proceed to give a little further

enlightenment. A Civil Servant's light was generally concealed under a bushel. Mr. Arnold Philip was one of those who had been fortunate enough to get a little light spread out from the interstices of the wicker basket.

No doubt Mr. Philip in his research work had had many cares that came upon him in connection with the routine work of his laboratory, and had suffered hindrances when engaged in some great piece of research, yet he thought that Mr. Philip would agree that it had been always an advantage to him on the whole to have before him the 'Fleet in being,' to be stationed at Portsmouth Dockyard with the naval architects and officers and engineers all round him, to be able to look out on Portsmouth Harbour, and cross over from Ryde, and see the battleships, which meant a good deal to him, because he could speak with authority about the composition of the mass of the metal they were composed of, and of all the difficulties of getting things up to the right specification from the steel in the sides of the battleships to the oil in their innermost bunkers. Mr. Arnold Philip was known to many of the members, so that he need not say any more about him except to call upon him to read his interesting and instructive paper.

Mr. Philip then delivered, in condensed form, the following paper, with lantern-slide illustrations :—

Some Laboratory Tests on Mineral Oils.

By Arnold Philip, B.Sc., A.R.S.M., F.I.C., *Member*.

Since its commencement in 1914 the Journal of this Institution has only contained two papers dealing with the description of methods for the technical chemical testing of Petroleum Oils. These papers were on: "The Estimation of Toluene in Crude Petroleum," by S. E. Bowrey, vol. iii, p. 287, and "The Testing and Standardisation of Motor Fuel," by E. L. Lomax, vol. iv, p. 6. As a prologue to the latter of these a letter from Mr. Alexander Duckham was read, the views in which largely explain the origin of the present contribution, and it is desirable to quote it *in extenso*. Mr. Duckham wrote :—

"Mr. Lomax's paper serves a very useful purpose in summarising in one published document so many examples of divergence in figures, resulting from the various methods of testing which are in vogue. It has, however, suggested to me an idea which I venture to think might have the consideration of the Council of the Institution, and which, I think, if acted upon, would result in considerable advantage to the consulting chemist, refiner and merchant, indeed to all those who are interested in the testing of petroleum products, and at the same time be a credit to our Institution, tending towards its recognition as *the*

authority on petroleum matters. My suggestion is that the Institution should appoint a Standardisation Committee whose reference should be to investigate methods of testing and to recommend standards for adoption in this country. At present, in even the simplest tests, carried out on well standardised apparatus, there will be found to be variations in results in the hands of different operators, this being due, of course, to small differences in manipulation, which have not been sufficiently detailed in the published instructions as to conduct of the tests.

Mr. Lomax has specially instanced the case of distillation. I would draw your attention to such minor tests as melting-points, boiling-points, viscosities, both on account of differences in manipulation, and of radical differences in the methods adopted. Then, again, there are different methods of expressing results: for example, some laboratories will express their viscosity-figures on a Redwood instrument in terms of the comparison with rape oil, whilst others give it in the number of seconds. Certain laboratories will report viscosities at 70° F., 140° F., and 200° F., others favouring 60° F., 100° F. and 212° F., and many other variations in these figures. In the determination of the cold-test, of the asphaltic content, of the liability to emulsification, and in the estimation of aromatics, and so on, it is seldom found that the methods of any two laboratories are strictly comparable. I feel that in this short letter I have dealt very inadequately with this most important subject, but I trust that what I have written will show what is in my mind, and that it may be considered of sufficient importance for the Council to decide to take action in the direction I venture to suggest. If such action were taken, I think it would be found advisable that the members of the Committee should be representative not only of the consulting chemist, but also of the Works chemist."

In April last Sir Boverton Redwood, whose recent death has been such an irreparable loss to this Institution, proposed to the present writer that he should contribute a paper to the Journal, dealing with some points in the technical laboratory examination of petroleum and other mineral oils. Sir Boverton's view was that although descriptions of novelties in methods of test would be of interest in such a paper, yet even in the absence of descriptions of any such new departures, a mere statement of the methods of procedure which had been found most satisfactory in dealing with difficulties met with in practice, would enable a further advance to be made in the discussion and criticism of the methods of oil testing, and possibly, by inducing the contribution of further papers of a similar scope from other oil technologists, might permit of some agreed standard methods for the technical examination of oil, such as had been foreshadowed by Mr. Duckham, being finally generally accepted.

In this connection it must be remarked that there are two policies which are advocated in the proposals which have hitherto been made for the standardisation of the conditions under which technical

chemical testing is carried out. The first of these is to standardise the method of test upon the lines proposed for petroleum testing by Mr. Duckham. The second policy is to standardise the composition of certain representative samples of each kind of material by means of elaborate check-tests carried out by analysts of acknowledged standing, the method of test employed being left to the choice of the individual analyst. The standard representative samples of material whose composition has been fixed by these tests are then to be available for distribution to chemists for checking their own methods. This latter procedure is that which has been decided upon by the Iron and Steel Institute with reference to the chemical testing of iron and steel, and the necessary investigations are now in process of being carried out.

A method of dealing with standardisation of chemical tests, which may perhaps be described as intermediate between the two above referred to, appears to have been decided upon by the conference held by the Institute of Chemistry on June 26th last, to consider the advisability of making provision for the preparation and issue of authoritative samples of chemical substances, metals, etc., of ascertained composition. This conference was attended by representatives of the Chemical Society, the Society of Chemical Industry, the Society of Public Analysts, the Institute of Metals, and the Federal Council for Pure and Applied Chemistry. The conclusions arrived at were that the provision of standard chemical substances was desirable, and that having regard to the fact that such standards could not be provided without reference to methods of analysis, the scheme should not aim at restricting the choice of methods, but always their revision and improvement.

The whole question of the standardisation of methods of test is complicated by the fact that every chemist accustomed to use a given method is naturally inclined to adhere to a procedure, the faults of which he recognises and for which he makes allowance, rather than change over to a new method, of which he has heard favourable criticisms, but concerning which he has had little or no practical experience. A change in method of test or method of report may further cause difficulty to the person to whom the report is addressed. It is true that all such inconveniences will be temporary, yet they nevertheless tend to give rise to a disinclination to adopt changes such as the proposed standardisation of methods of test must cause.

The author is at present hardly prepared definitely to advocate any one of these three policies for the standardisation of chemical testing as applied to mineral oils, but is inclined to consider that

for such materials the standardisation of methods as proposed by Mr. Duckham, will be found to be necessary, on account of the fact that the large quantity of a sample which is required for a single complete set of tests on an oil must render it practically impossible to preserve standardised samples for issue to analysts generally for carrying out comparative tests under routine conditions. For steels and other metals and alloys, the storage and issue of standardised samples can be comparatively readily arranged. A complete analysis in duplicate of a steel, however complicated its composition may be, can be carried out on a weight of three ounces, whilst for the routine complete test of a mineral oil a sample measuring at least a quart (i.e. about 40 ounces) is required. Moreover, the storage and transmission of samples of oils obviously present difficulties which do not exist in the case of samples of metals and alloys.

One objection to the standardisation of methods of test as opposed to the preparation of samples of standard composition, by means of which samples each analyst can standardise his own method, is more serious than those referred to above, and consists in the real difficulty which is experienced in describing any method of test in such a way that different operators having different experience in testing will all apply it in the same manner. It is believed that most chemists who have had experience of the difficulties in testing on the large scale must have realised that although a method of test used in a given laboratory may as a general rule be carried out in the same manner, yet even under such conditions, with a large staff of analysts, the members of which change from time to time as resignations and fresh appointments take place, a good deal of careful supervision is required in order to secure that the routine of testing is carried out in all its necessary detail, even although full written instructions for the methods are supplied for information. Usually, these must be supplemented by direct verbal instruction.

If this is true of one large laboratory, it must prove a still more difficult matter to secure uniformity of method of test by means of written instructions in all laboratories. In any case, as Mr. Duckham has indicated, it will be necessary to describe all such accepted methods in a particularly detailed manner. Difficulties are made to be overcome, but for this they must be recognised, and there can be no doubt that the preparation of such elaborate descriptions as appear to be required will be found a tedious matter, and probably the perusal of the methods will prove still more so to the generality of chemists.

It is evident that the present contribution cannot be of the

nature of a systematic treatise on the testing of mineral oils. The space available makes it possible only to deal with a small section of the methods of test in use. The particular methods of test described, and the record of difficulties encountered, are limited to those which, as far as the author is aware, possess a certain degree of novelty, and have not been treated in text-books or current technical literature, and it is hoped that the discussion of their merits and demerits may prove of some use as tending to crystallise views on the question of the ultimate standardisation of the chemical testing of such materials, whether it is finally decided to standardise the methods of test as proposed by Mr. Duckham, or to standardise the samples available for test on the lines adopted for steel testing by the Iron and Steel Institution, or as proposed by the Institute of Chemistry.

It will be noticed that in certain of the results given in the following pages, temperatures are expressed both in the Fahrenheit and also in the Centigrade scales, even in a single report on one sample of oil. This apparently inconsistent procedure is followed by most analysts, and is due, so far as the present writer is guilty of it, to the fact that in many if not most existing specifications for petrol and other refined oils, the temperature-limits are fixed for one test in Fahrenheit degrees, and for another on the Centigrade scale.

In this and in other similar respects custom lies heavy on us all, and it is believed that little short of an authoritative pronouncement such as can be made by the Standardisation Committee, the formation of which Mr. Duckham has suggested, can overcome such absurdities, and it is thought that besides the standardisation of either samples or methods, a most important function of the Committee may well be the standardisation of specifications for most of the more largely used products of oil distillation. Even where a detailed specification is not desirable, it may be possible to fix the general form and method of specification with great advantage to the buyer, the vendor and the tester of mineral oils and of the distillation-products of shale and coal, and also of power alcohol.

As a preliminary to the discussion of methods for the chemical testing of oils, it is necessary to make some remarks upon the method of preparing samples for laboratory tests.

It must be assumed in the first place that the bulk sample of oil, as received in the laboratory, really represents correctly the contents of the land storage or steamer-tank from which it has been drawn, and it is observed that the drawing up of full directions for the sampling of large tanks of oil will probably form another useful

branch of the duties of any Standardisation Committee which may be formed.

Unless special precautions are taken, the preparation of small representative testing samples of oil fuels or other dark-coloured oils, creosotes and tars from their bulk-samples as received in the laboratory, sometimes presents considerable difficulties, on account of the want of homogeneity of the material, a characteristic which cannot be observed by direct inspection. Such liquids may consist of mixtures in varying proportions of water and wet oil or tar, and emulsions of water and oil, or water and tar.

Generally, if an attempt is made to obtain average samples for test from such oils by shaking up the bulk samples, it is found that in the operation of subsequently endeavouring to pour off a portion for test, some of the water again separates out in the bulk-sample, and consequently the object of obtaining a representative sample is defeated.

Under these circumstances the author is accustomed to adopt a routine procedure for preparing representative samples for test for all such opaque or dark-coloured oils, creosotes and tars, and it cannot be insisted upon too strongly that, unless satisfactory precautions of this character are taken, all tests made on such oils may be to a large extent illusory.

Some such procedure as that described below must therefore always be carefully followed, in order to obtain a sample upon which any valid test can be carried out.

The specification of the necessary precautions is somewhat tedious to set down in words, but the actual practical operations are in reality quite simple to perform.

The procedure of preparing representative samples for test need not generally be rigidly followed in the case of transparent oils, whether these are motor-spirits, kerosenes, creosotes or crude oils, for when an oil is transparent, the amount and distribution of any separated water or emulsion can be observed by inspection, and a more convenient and shorter method of measuring the separated water and preparing the representative sample may clearly be usable. In all cases, however, it is necessary to prepare representative samples by some suitable procedure.

In order to avoid any ambiguity in reporting the results of tests made upon samples of oil fuels, creosotes, crude oils or tars, etc., it is the author's custom to make the following statement on the certificate of test—"All the results of tests given above, except the water determinations, are reported as having been obtained on the oil as separated from any visible water or emulsion"—and for the

sake of uniformity this form of report is used even when no water or water emulsion has been separated.

The methods of preparation of samples for tests used by the author are as follows:—

The Preparation of Representative Testing Samples of Oil Fuels, Creosotes and Tars.

1. *Liquids possessing a density less than that of water.*—The bulk sample on receipt is allowed to stand in the laboratory in its containing vessel at the room temperature without shaking for at least 17 hours, i.e. overnight. The vessel is then tilted, so that any separated water may collect together in an increased depth at one side at the bottom. The end of a long-stemmed 100 cc. pipette is next inserted, so as to reach to the bottom of the vessel at the side where the water has been caused to collect, and a quantity of the sample measuring about 100 cc. is withdrawn for inspection. If no water is observed in this sample it is returned to the vessel. The whole sample is then thoroughly mixed by shaking, and the amount necessary for carrying out the requisite tests, designated *Sample A* in the following paragraphs, is withdrawn and tested as desired.

If, however, visible water, other than small drops adhering to the pipette, is observed, the 100 cc. is returned to the containing vessel, and the whole of the bulk-sample is again allowed to stand and settle for a further period of about 17 hours. At the end of this second period three-quarters of the settled bulk-sample is cautiously decanted off, all shaking being as far as possible avoided. This decanted three-quarters of the original bulk-sample is designated *Sample B*. The remaining one-quarter of the bulk-sample left in containing vessel, designated *Sample C*, is then poured into a large separating funnel and the *Sample B* which was first decanted off, and which has thus been freed from the greater portion of the larger bubbles of water is returned to the original vessel and well shaken up, and then like *Sample A*, used for the necessary tests.

The *Sample C* containing most of the separated water and emulsion is allowed to stand in the separating funnel for 17 hours, when all the visible emulsion and water, and about one-quarter of the remaining oil, is run off from the funnel, and mixed with an equal volume of kerosene, in order to cause a more complete separation of the water. After this kerosene mixture has been allowed to stand overnight the volume of the separated water is measured, and this, together with the mixture of oil and kerosene, is transferred to a properly labelled bottle and retained for future reference if

necessary. The amount of water found in *Sample C* as described above is calculated as a percentage on the whole of the original bulk-sample, and reported as *Visible Separated Water*.

II. *Liquids possessing a density equal to or above that of water.*—Creosotes and tars usually possess a density greater than unity, and the separation of water by gravity from such liquids either does not take place at all, or is extremely slow. This is particularly observable with coal-tar creosotes and low-temperature coal-tars, whose density is only slightly greater than that of water. In transparent creosotes large globules of water may sometimes be observed immersed in the creosote, and neither rising or falling. In tars which have been obtained by high-temperature distillation, and which have higher gravities, the separation of the water on standing usually takes place at the top of the sample.

For creosotes and low-temperature tar the best method of preparing a representative sample is to proceed as for *Sample A* in Section I above.

For dense tars a suitable modification of the procedure described in Section I may be employed, making the necessary alterations indicated by the water being separated on standing at the top instead of at the bottom of the sample.

Distillation Tests on Crude Oils.

Mineral oils, the testing of which is normally dealt with in the chemical laboratory, may be divided roughly into the three following groups:—

I. Petroleum Oil Group. Including	II. Shale Oil Group. Including	III. Tar Oil Group. Including
a. Crude petroleum.	*a*. Crude shale oil.	*a*. Tar.
b. Petrol spirit.	*b*. Shale spirit.	*b*. Benzole.
c. Turpentine spirit.	*c*. Shale burning oil.	*c*. Middle oil.
d. Kerosene.	*d*. Shale gas oil.	*d*. Creosote oil.
e. Solar oil. Gas oil. Spindle oil.	*e*. Shale lubricating oil.	*e*. Anthracene oil.
f. Mineral lubricating oil.	*f*. Paraffin wax.	
g. Petroleum residuum oil.		
h. Paraffin wax.		

It is important to notice that the problem of testing the crude oils of each of these three groups is radically different in its nature from that involved in the testing of the other members of the

groups. That this is so, may be readily recognised when it is considered that the partially or completely refined members of Group I, marked *b* to *g*, are supplied to some more or less definite specification, and they are therefore tested to ascertain whether they conform to this specification or not. But crude petroleum is a natural product, the composition of which is in no way controlled by human agency.

The technologist has therefore to accept the crude oil as it is won from the earth, and to do the best he can with it to obtain by distillation larger or smaller amounts of secondary derived products which are directly useful commercially. The cuts in the distillation, when the oil is refined on the manufacturing scale, must be so chosen as to give secondary products agreeing in properties with some generally accepted although perhaps somewhat broad specifications for petrol, kerosene, Diesel engine oil, spindle oil, gas oil or lubricating oils of various classes, paraffin wax, or, finally, heavy fuel oils as the case may be. What is true of crude petroleum oil is also largely true of crude shale oil and coal-tars, for these primary products of the distillation of oil shale and of coal respectively are practically fixed in character by the manufacturing processes by which they are obtained, and when once these processes have settled down to their most economical form, crude shale oil and tars, whether derived from the shale oil, coal gas, blast furnace smelting, metallurgical coke, or coalite industries, become practically as fixed in character as are the natural crude petroleum oils obtained from as many different oil fields. Laboratory tests on the natural crude oils, and also, if one may so term them, on the semi-natural crude oils from coal and shale, are therefore directed to ascertain what proportions they contain of oils or other secondary technically useful fuel distillation products, and merely consist in the first instance in submitting them to fractionation tests on the small scale. From the results of these distillation tests, an opinion must be arrived at as to the probable best distillation treatment to which the crude oils can be submitted on the large scale in order to obtain the most satisfactory financial results.

The final decision as to the best method of fractioning a crude oil on the large scale must clearly be decided in any given case by the market prices obtainable for the products, and these for a given oil may change with the times. Examples of such changes in value are familiar to all as represented amongst others by the variation in the value of petrol, benzole, fuel residuum oil, anthracene oil, paraffin wax, tar and pitch as caused by improvements and inventions in internal-combustion engines, oil-heated boilers, dyeing

processes, methods of refrigeration, road-surface treatment, etc., etc.

It is useful to remember that there is no known method of fractional distillation by means of which two or more mixed or dissolved liquids can be completely separated from one another by a single operation. All methods of fractionation are, however, attempts to approach as closely as possible to the ideal condition of complete separation so far as practical conditions will permit.

The amount of success which will attend such attempts depends upon every detail in the method of procedure.

Methods of fractionation are therefore arbitrary, and are endeavours to approach an unreachable ideal; if the details of the method are varied, the degree of approach to the ideal must also vary.

For technical testing purposes it is of the greatest importance that a method of fractionation shall be selected which will give the most uniformly concordant results when the same sample of mixed liquids is submitted to it by repeat tests made either by the same or by different operators. It must in fact give the most concordant results under all conditions of use, and must depend as little as possible upon the personal experience and skill of the operator. It is also clearly most desirable that standard methods of fractional distillation tests shall occupy as short a time to carry them out as is possible, consistently with the condition that satisfactorily accurate and uniform results are obtained. Given a sufficient degree of accuracy, it is also desirable that such tests shall be capable of being carried out on small volumes of liquid, because tests on larger samples occupy more bench room, and give rise to inconvenience in their transmission, storage and handling, and if they are of an inflammable nature the fire-risk is correspondingly increased when dealing with large samples. It, moreover, not infrequently happens that only small samples of a liquid which it is desired to submit to fractionation are available, and a method of testing which will allow of the use of small volumes is therefore desirable from this point of view. A further fact which must be taken into consideration when dealing with the fractionation of liquids which are liable to be partially decomposed or cracked at or near their temperature of volatilisation, such as the heavier petroleum tar and shale oils, is that the method of distillation used should as far as possible avoid this cracking action. Lastly, it is desirable that the results of the laboratory fractionation of an oil should give results which approach as nearly as possible to those which will be obtained when the oil is distilled on the large scale in a refinery.

In the following pages wherever methods of fractional distillation are described it must be understood that the details given represent what the author's experience has shown to satisfy most closely the sometimes opposing claims outlined in the foregoing considerations. It is to be hoped that by the contribution of other experimenters it may be possible, in the early future, to arrive at a conclusion which will permit of the general acceptance of methods of fractionation both for crude petroleum oils, shale oils and coal tars, and also for creosotes, petrols and kerosenes. Such accepted methods of test can, it is considered, only be decided upon from a careful revision of the results of large numbers of tests carried out by their means by different workers.

The degree of uniformity arrived at in such series of tests must, it is believed, afford the most reliable guide in deciding upon the acceptance of any given process as the standard. The selected process will probably not embody all the details of any known method now existing, but will be built up by the judicious selection and combination of details from many or all. The writer is accustomed to carry out the laboratory distillation-tests on crude oils or tars in two operations, each performed in a special apparatus. In the first of these the oil is submitted to a topping distillation up to a temperature of 500° F. (260° C.), by means of which, in the case of a crude petroleum oil, the petrol, water and kerosene are separated together. This preliminary topping distillation is carried out on a quantity of 1,000 cc. of the crude oil or tar, heated in a glass distillation-flask of about 1,500 cc. capacity. During the whole of the distillation a current of 2·4 cubic feet of air per hour is sucked through the heated liquid by means of an electric motor-driven air-pump. The current of air and vapours of distillation-products are cooled down after leaving the still by passing through a water-cooled condenser; this separates the condensible vapours, and the air, containing only traces of vapour, is then once more heated, and passed through the hot oil, and thus continuously circulates until the distillation is complete. The vacuum in the distillation-flask throughout this operation is about eight inches of water.

The arrangement of the apparatus used is shown somewhat diagrammatically in Fig. 1. The distillation-flask A, which contains the sample of oil or tar which it is desired to top, is contained in an air bath B, and is surrounded by a coil of $\frac{3}{16}$-inch i.d. copper tube L, which acts as an air-heating coil. The flask is closed by an india-rubber bung through which pass a thermometer *t*, a leading-in tube 1, which is constricted at its lower end, and a leading-out tube 2. The leading-out tube 2 is connected directly

to one limb of a Y-shaped condenser C, which is provided with a tap at T (not shown in the figure) and is surrounded by a water jacket D, through which water may be kept circulating, or if desired cooling may be caused by means of packing the jacket with pounded ice. The other limb of the Y condenser is connected by a tube to the exhaust side G, of an electric driven air-pump F. The pump used by the author is a Lennox blower. The compression side H of the air-pump is connected to the apparatus K, which is a gas current-meter and indicates the volume of gas passing through it by readings on the oil or water pressure-gauge at M.

After passing through the gas current-meter, the air passes through the heating coil which surrounds the flask A, and then again passes through the hot liquid in the flask, thus continuously circulating round the system during the whole duration of the distillation. The distillation-flask and heating-coil are heated by the burner N. The magnitude of the current of air passing through the oil is measured and controlled by the gas current-meter K, and controlled by means of the two screw clips P and Q. P is a clip on the main circuit, and Q is on a by-pass tube connecting the exhaust to the compression side of the air-pump. By means of these two clips, the adjustment of the volume of air flowing round the circuit, as also of the vacuum in the distillation-flask and condenser can be readily made. The amount of vacuum is indicated by means of the height of the column of water in the tube R. The arrows indicate the direction of the flow of the air through the system.

The operation of topping may have two objects in view: it may be carried out in order to ascertain the amount of water, petrol and kerosene or benzol and naphtha in an oil or tar, as the case may be, or it may, as is sometimes necessary, be directed to ascertaining what amount of topping must be removed from a crude or partially crude petroleum oil or creosote in order to raise its flash-point to some minimum value which is prescribed by the specification for a particular class of oil fuel, and thus to find the amount of fuel oil residuum of a particular specified minimum flash-point which can be obtained from the crude oil. If the crude oil is being topped for this latter purpose, the operation is carried out by trial and error.

The method of topping here described, when used for removing the petrol, water and kerosene from a crude oil, or naphtha and water from a tar, has the advantage that it does not require to be varied for any particular oil or tar. The conditions as to the magnitude of the air current, the vacuum in the distillation-flask and the

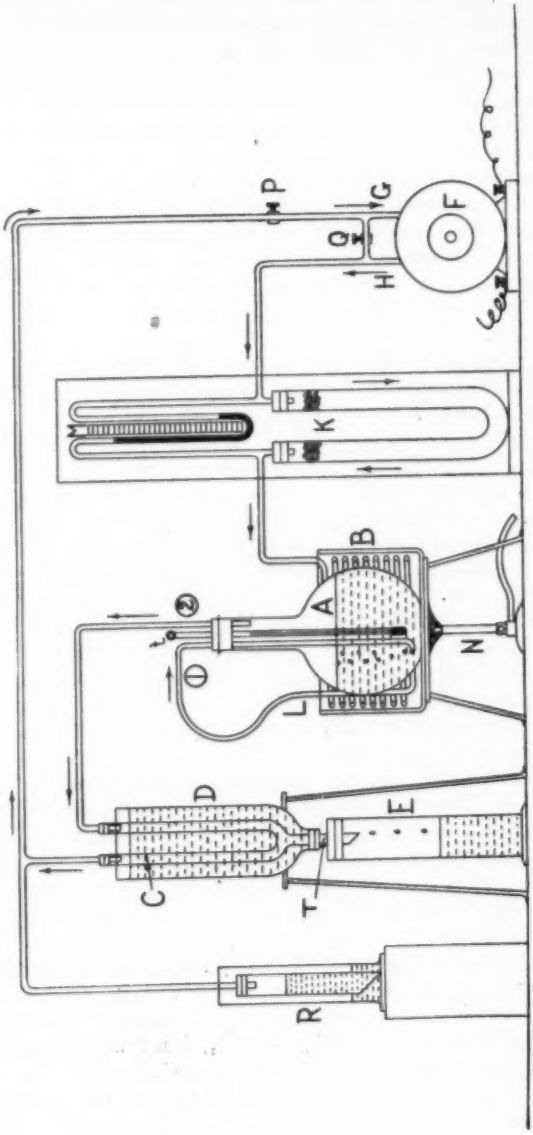


FIG. 1.—Topping in air-current.

maximum temperature of the oil, are all fixed in a simple manner, and the progress of the distillation requires very little attention, and is completed much more quickly than a direct distillation, whilst, as the circuit forms a closed system, the loss of volatile constituents is very slight. In distillations of some oils without the current of gas, water-globules are apt to condense on the neck and sides of the still, and when these fall back into the oil below, after it has been heated above the boiling-point of water, they cause inconvenient and sometimes dangerous explosions. When the current of air or gas is used, no globules of water condense inside the flask, and this source of trouble is therefore avoided. When tars are distilled by this method, no trouble from frothing is experienced.

It is, perhaps, desirable here to describe briefly the gas current-meter K. This apparatus, which has also been termed a gas-flow meter, consists of a glass U tube filled with small lead shot. The gas flowing through the current-meter passes through this column of lead shot, and the resistance to its flow involves as a necessity that a definite difference of pressure exists at the two ends of the column. This pressure is some direct function of the gas current passing, and each reading on the oil-filled pressure-gauge at M can therefore be made to indicate a definite gas current through the meter.

In order to ascertain what the readings on the gauge signify, the current-meter and an ordinary suitable experimental gas-meter are connected in series, and a steady current of air is blown through the two instruments, and adjusted by a fine adjustment-valve until the quantity and time readings on the gas-meter indicate that a steady current of the desired value is passing—let this be, say, 2·4 cubic feet per hour: the reading on the oil pressure-gauge scale of the current-meter is then recorded, and whenever the oil gauge gives this reading, it is clear that the current of gas passing through the current-meter must be 2·4 cubic feet per hour. The reason that shot are used, instead of sand or other inert powder in the resistance column of the gas-current meter, is on account of the fact that the shot do not vary in their form of packing into the tube. That this is the case can be readily shown by taking the shot out of the pressure-tube, after the gas-current meter has been calibrated, and then returning them and again testing the current-meter in comparison with the experimental gas-meter, when it will be found that the reading of the oil gauge remains unchanged for the same gas-current. Sand-packing does not possess this convenient property.

This form of gas-current or gas-flow meter is very cheap to make, and can be constructed with varying forms of resistance column or tube, and of pressure-gauge, depending upon the gas current which it is desired to measure. The writer has used it for currents varying from 50 cubic feet down to a tenth of a cubic foot per hour, and no doubt larger or smaller currents can be easily measured if desired. For very small currents the necessary resistance tube is made of several feet of capillary glass tube or of a porous plate, and for the larger currents glass or metal tubes of ¼-in. diameter can be used, and no shot packing is required. In these flow-meters it is only necessary to make a single direct reading to ascertain the gas current, whereas in the expensive experimental gas-meters, which are actually quantity-meters, a double reading of both time and of the quantity of gas must be made in order to ascertain the current flowing through them at any given time. It is therefore not possible with such quantity-meters to observe at once whether the gas current is changing or not. The current-meters as here described are believed to be novel, and they are cheap and easily constructed, have a very wide range of current-magnitude, and when in use immediately indicate any change of flow, which can therefore be adjusted at once by a suitable control-valve. In all cases where specially accurate adjustment and measurement of gas-currents is aimed at, it is desirable to insert in the circuit a cone-seated screw tap or valve having a fine adjustment, but for most purposes a screw compression-clip on a rubber tube is all that is necessary for purposes of regulation. Current-meters of this type can be made and calibrated for laboratory use for practically any magnitude of gas-current within a couple of hours at a very trifling cost, provided a satisfactory experimental quantity-gas meter is available for purposes of calibration. The pressure-gauge used in such gas-current meters, if of the liquid form, must be provided with a suitable level, to secure that it is always read in the same position as that which it occupied when the gas-flow meter was calibrated.

In order to carry out the process of topping as described above, the air-current is started and adjusted, the burner is lighted and the temperature of the crude oil or tar is raised as rapidly as possible until the distillate is seen to come over at the rate of about three drops per second. The distillation is then continued at this rate by suitably increasing the supply of heat until the temperature of the oil in the flask rises to 500° F. (260° C.). The operation is then stopped, and the amount of distillate, which consists of petrol and kerosene or benzole and coal-tar naphtha, as the case may be,

with water, is measured by weight, and from the specific gravity of the mixed oils the volume is calculated, suitable correction being made for the volume of water present. The mixture of kerosene and petrol or benzole and coal-tar naphtha is then fractioned. The petrol, benzole, kerosene and naphtha fractions are finally separately examined to ascertain their quality.

The second stage in the fractionation of the crude oil is next carried out on the residue left in the flask A (Fig. 1), after the topping has been completed.

For this purpose it is convenient to use a still of small capacity for laboratory tests, both on account of the smaller space taken up by the still, and also because samples of oil available for laboratory tests are often of rather small volume. On the other hand, the larger the sample distilled, the more closely the results obtained can be considered as giving an accurate representation of what will occur on the practical scale in the refinery. These conflicting requirements must be reconciled as far as possible by some compromise.

The laboratory still which the author has devised has been found to give satisfactory results when dealing with not more than 700 to 800 cc. of the topped oil. This still is believed to be a new development both in its form and in the principle on which it acts. It is shown in sectional elevation in Fig. 2. The body and lid are made of iron, steel or copper, and the lid is attached to the body by bolts passing through wide and thick flanges, the joint between which may be made quite gas-tight by means of a washer of some steam-packing material. The body of the still is made of a smaller lower cylindrical portion A, fitting into a larger upper cylinder B. The radial space between the two cylinders where A projects into B has the form of an annular gutter F, into which all oil-vapours condensing on the sides of the cylinder B drain, as also do any vapours which are condensed on the inside of the dome-formed lid C, and which may run down the sides of the cylinder B.

When the charge of 700 to 800 cc. of oil is placed in A, the level of the upper surface of the oil is $2\frac{1}{2}$ inches from the upper edge of A. The slightly coned disc-formed thin sheet-iron baffle-plate D, which is pierced with holes, is supported on three sheet-iron legs, which stand on the bottom of the still. The edge of the upper surface of this baffle-plate is about $\frac{1}{4}$ inch below the upper edge of A, and $2\frac{1}{4}$ inches above the surface of the oil. The holes through the baffle-plate are pierced from above, and the burred edges of the metal round the holes point downwards. This arrangement of the holes facilitates the return, into the oil under distillation, of any

oil falling upon the plate from the upper baffle-plate, and also of any splashes of oil striking against its under surface. The oil vapour which passes through these holes then passes through a second upper conical perforated baffle-plate E, in which the holes are pierced so that their edges point upwards. This second baffle-plate fits by a flange G, like a lid, into the top of the lower cylinder A. Any oil splashes striking the under side of the baffle-plate F flow down the sloping under-surface of the cone, and again unite with the oil in the still below, but any vapour of oil which condenses outside E runs down its outer surface into the annular gutter F, and there runs off viâ the eduction pipe H, which is kept

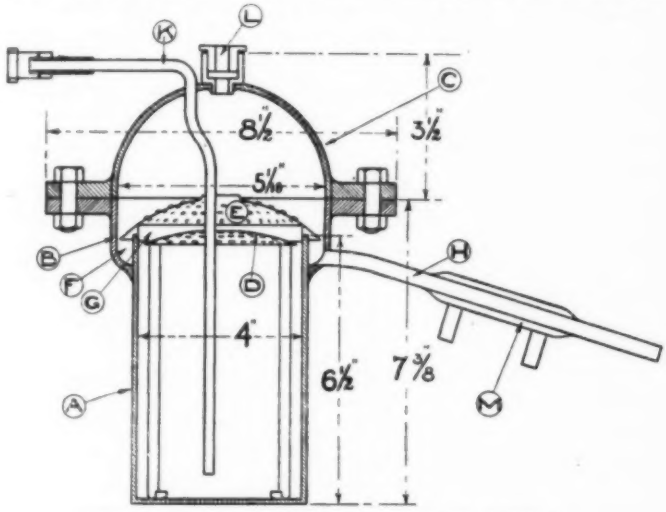


FIG. 2.—Laboratory Still for Fractionation at 250° C.

cool by a water jacket M, and together with any uncondensed oil-vapour and steam, passes to the condenser, which consists of a glass Y tube provided with a tap, and surrounded by a water-jacket similar to that shown at C in Fig. 1.

Gas or superheated steam, or both, may be passed through the oil in the still during the distillation by the pipe K, and the magnitude of the current of gas can be observed by means of an oil tell-tale bottle. A thermometer passes down into the oil through the stuffing box L. Both the pipe K and the thermometer, which is not shown in Fig. 2, pass through holes cut in the middle of the baffle-plates E and D. The still is heated by

a powerful Bunsen burner, and is well lagged with asbestos cloth over the upper portions. This form of still is easy to clean. The fact that, when once the oil vapour has passed through the two baffle-plates, it is impossible for it to return into the still, increases the output of the still, and tends to prevent or reduce cracking. Owing to the fact that the baffle-plates are made of thin metal, have small heat-capacity, and are close to the surface of the oil, little or no condensation takes place on them; they merely act as separators of oil in the liquid form from oil in the form of vapour. The splashes are returned to the liquid oil in the still, whilst when once outside the upper coned baffle-plate any vapour which becomes condensed is unable to return, but runs down into the annular gutter, and is carried away by the eduction pipe. The fact that the total effective cross-section of all the holes through the baffle plates is large, causes the velocity of the vapour passing through them to be small, thus tending to prevent unvaporized drops of oil being carried over into the distillate. In a recently constructed still of this type, the thermometer is placed in a copper tube at the bottom of the still, and the steam pipe also passes through the side of the still as shown in Fig. 3, whilst the oil and vapour-separator is of a louvred form. This form of construction is rather more satisfactory in its action than the first form described.

Table I gives the results of laboratory distillation of one particular sample of crude petroleum oil as obtained by the author and by four other experimenters of eminence, two series of results being given by the observer E. The results shown in the column B were obtained by the author with the stills shown in Figs. 1 and 2. It is interesting to note that all the results of these five different observers were obtained quite independently, and the limits of temperature or gravity at which the cuts were made were also selected independently. The volume taken for distillation in these tests is only known in three cases, namely those of B, E (1), and E (2). There is reason to believe that the volumes taken by the observers A, C and D, were considerably larger than 2,000 cc. On the whole it is considered that these results of the laboratory-distillations of crude oil are substantially similar in character, and this remark especially refers to those obtained by the observers A, B, C and D. The combined loss by distillation and by chemical treatment in the tests made by A, B and D, are very satisfactory, and appear to indicate that any cracking action must be small in so far as the formation of gaseous products is concerned. It is of interest to note that the paraffin wax present in the oil (item 8)

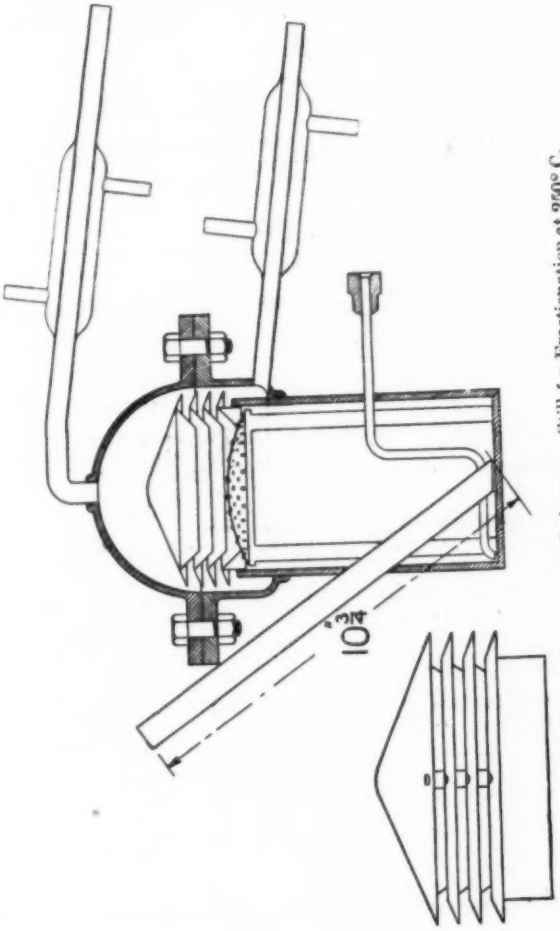


FIG. 3. – Modification of Laboratory Still for Fractionation at 250° C.

Table I.

Tabulated results of the quantitative fractional distillation of the same independent observers A, B, C, D and E, and also of the Hardstoft Boring

	Observer.	A.		B (Author).	
	Volume of Original Crude Oil Distilled.	———		1938 cc.	
Item No.	Description of distillate.	Constants of distillates.	Vol. per cent.	Constants of distillates.	Vol. per cent.
1	Petrol	Dist. betw. 40° and 150° C. S.G. = 0·735	31·7	Dist. betw. 42·5° and 150° C. S.G. 0·737 @ 60° F.	33·4
2	Turpentine spirit or white oil	Dist. betw. 150° and 160° C. S.G. = 0·779	5·0		
3	Kerosene ..	Dist. betw. 160° and 270° C. S.G. = 0·806	29·7	Dist. betw. 150° and 260° C. S.G. 0·803 @ 60° F.	29·5
	Total petrol, white oil and kerosene fractions	.. .. ..	66·4	.. .. ..	62·9
4	Gas oil, solar oil or Diesel engine oil ..	Dist. betw. 270° and 300° C. S.G. = 0·845	7·6	Dist. betw. 260° and 340° C. S.G. 0·8540 @ 60° F.	19·32
5	Light lubricating oil	S.G. 0·875 @ 15·5° C.	7·0		
6	Medium lubricating oil ..	S.G. 0·895 @ 15·5° C.	6·1	Dist. betw. 340° and 355° C. (solid at 65° F.)	6·32
7	Heavy lubricating oil ..	S.G. 0·900 @ 15·5° C.	6·7	Dist. betw. 355° and 395° C. (solid at 65° F.)	6·00
8	Paraffin wax ..	Crystalline M.P. = 114° F.	2·5	Crystalline from:– Item 6, M.P. = 121° F.	0·38
				Item 7, M.P. = 134° F.	0·59
9	Tar or pitch ..	Hard pitch ..	1·6	Soft pitch ..	4·02
10	Loss by distillation	.. .. ..	0·6	Loss by distillation	0·47
	Loss by chemical treatment	.. .. ..	1·5	Loss by chemical treatment	nil
11	Total	.. .. ..	100·00		100·00

TABLE I—*continued.*

sample of crude petroleum oil (marked 3162 A/18) as recorded by five crude oil by the author.

C.		D.		E (1). 2000 cc. a	
Constants of distillates.	Vol. per cent.	Constants of distillates.	Vol. per cent.	Constants of distillates.	Vol. per cent.
Spirit S.G. 0·7095	9·90	Dist. betw. D.P. and 150° C.	35·0	Dist. betw. 55° and 148° C.	30·0
Spirit S.G. 0·7395	10·01	S.G. 0·734 @ 60° F.		S.G. 0·730 @ 60° F.	
Spirit S.G. 0·758	7·65				
	27·56				
S.G. = 0·804 ..	35·05	Dist. betw. 150° and 250° C. S.G. 0·8085 @ 60° F.	25·0	Dist. betw. 148° and 300° C. S.G. 0·808 @ 60° F.	40·5
	62·61		60·0		70·5
Light gas oil S.G. = 0·848 Viscosity @ 70° F. = 42 seconds.	6·20	Dist. betw. 250° and 290° C. S.G. 0·859 @ 60° F.	10·0	Dist. betw. 300° C. and X. S.G. 0·855 @ 60° F.	7·7
		Pale machinery oil. S.G. 0·880 @ 60° F.	5·6		
Fuel oil S.G. = 0·8795 Viscosity @ 70° F. = 119 seconds.	19·64			Dist. temperature not stated X. S.G. 0·900 @ 60° F. Classed as spindle oil.	16·7
		Dark summer machinery oil. S.G. 0·923 @ 60° F. This is the residual oil, containing all the tar and paraffin wax.	22·5		
From item 6, M.P. = 105° F.	2·26			Paraffin wax separated from items 5, 6 and 7 by Holde's method.	2·9
Residuum ..	3·65			Pitch and loss ..	2·2
Total loss by distillation and chemical treatment	5·64	Total loss by distillation and chemical treatment	1·9		
	100·00		100·00		100·00

TABLE I—*concluded.*

	Observer.	E (3).		Results of Tests on Hardstoft Oil by Author.	
	Volume of Original Crude Oil Distilled.	2000 cc.		1214 cc.	
Item No.	Description of distillate.	Constants of distillates.	Vol. per cent.	Constants of distillates.	Vol. per cent.
1	Petrol	Dist. betw. 54° and 169° C. S.G. 0·728	29·2	Dist. betw. 45·5° and 150° C. S.G. 0·7155 @ 60° F.	4·8
2	Turpentine spirit or white oil	Dist. betw. 138° and 218° C. S.G. 0·781	16·2		
3	Kerosene ..			Dist. betw. 150° and 260° C. S.G. 0·7790 @ 60° F.	37·5
	Total petrol, white oil and kerosene fractions		45·4		42·3
4	Gas oil, solar oil or Diesel engine oil			Dist. betw. 260° and 340° C. S.G. 0·8385 @ 60° F.	28·47
5	Light lubricating oil			Dist. betw. 340° and 355° C. S.G. 0·8752 @ 60° F.	7·18
6	Medium lubricating oil			Dist. betw. 355° and 420° C. S.G. 0·8772 @ 60° F.	10·94
7	Heavy lubricating oil	Total residual as oil fuel and loss. S.G. 0·860 @ 60° F. F.P. 145° F. Viscosity at 32° F. on pattern II, Redwood viscometer 385 seconds.	54·6	Dist. betw. 355° and 495° C. S.G. 0·8822 @ 60° F.	6·67
8	Paraffin wax ..			Paraffin wax separated from items 4, 5, 6 and 7. M.P. = 110° to 129° F.	1·96
9	Tar or pitch ..			Coke	0·52
10	Loss by distillation Loss by chemical treatment			Distillation loss	0·26
11	Total		100·00		100·00

is estimated as varying from 0·97 by the author, up to 2·9 by E, whilst the melting-point of the separated paraffin varied from 105° F. as found in 2·26 % of wax by C up to 121° F. and 134° F. found on 0·38 % and 0·59 % respectively obtained from the items 6 and 7 by the author. In this connection the following comments made by the various observers on the paraffin wax present in this oil are of interest:—

A states that the paraffin wax is present to the amount of 2·5 % of the crude oil. This wax is yellowish-white in colour, is crystalline and possesses a melting-point of 114° F.

B, the author, states that the paraffin wax in the fraction from 340° C. to 355° C. (item 6) amounts to 5·66 % (0·38 % calculated on original crude oil). This wax is crystalline, and has a melting-point of 121° F., whilst the fraction distilling between 355° C. and 395° C. (item 7) contains 9·13 % by weight of paraffin wax (0·59 % calculated on the original crude oil). This wax is also crystalline, and has a melting-point of 134° F. He adds that it appears that the paraffin wax which occurs in these two fractions is of a satisfactorily high melting-point, and can be observed in the oil in the form of crystals. It cannot be stated as to whether it would be a paying operation to recover it.

C states that an attempt was made to obtain paraffin wax from the heavy oil (item 6), and adds that on account of the character of the crude petroleum, the ordinary method which had been successfully employed in the case of other petroleums was found to give discouraging results, and it could not be stated that as a practical industrial and economic proposition the fraction item 6 could be so dealt with. Not only does this fraction, when cooled to a low temperature and filtered, filter very slowly (on account of its viscosity and its bituminous character), but the crude solid paraffin so obtained is pasty and comparatively amorphous or non-crystalline in structure, thus making its extraction a tedious process. Moreover, when submitted to the sweating process for the production of refined wax, the crude solid petroleum refuses to part freely with the contained oil, with the result that almost the whole cake has to be melted away, and even then no good-coloured wax is obtained. The actual treatment of the heavy oil (item 6) yields 2·26 gallons of crude solid paraffin of M.P. 105° F. per 100 gallons of the original crude petroleum. The crude solid paraffin with its adhering oil had a melting-point of only 105° F. That this low melting-point is due not only to the presence of oil, but also to the fact that the petroleum contains no solid paraffin of the ordinary high melting-point, is shown by the result obtained in

the sweating process, by means of which only 0·10 gallons of wax, of a melting-point of 112° F., was obtained from 100 gallons of the original crude petroleum.

D states that the oil contains only traces of bitumen and a small percentage of paraffin, which he considers it would not pay to extract. No determination of the amount present was reported.

Finally, E states that the 16·7 % distillate obtained above 300° C., consisting of lubricating oil and paraffin wax, possessed a gravity of 0·900 and was solid at the ordinary temperature, and he adds that the presence of paraffin in the residual pitch was evident from its appearance. The paraffin wax was separated from the lubricating-oil fraction by Holde's process, and was found to amount to 2·5 % by weight calculated on the original crude oil.

The variations in the results of the determinations of the amount and character of the paraffin wax in crude oils, as shown by the above reports, is considered to be due, firstly, to the different methods of separation employed; and, secondly, to the greater or less efficiency of the distilling process in preventing the cracking of the oil and, what is equally important, their efficiency in preventing bitumen and vaseline passing over into the distillate by priming. In this connection it is considered that the high melting-point and the crystalline character of the paraffin wax separated by the author by means of the special still used, indicates the efficiency of the operation of this apparatus, and this is especially noteworthy when it is considered that the whole distillation-test was carried out on so small a quantity as 720 cc. of the topped oil (equivalent to 1,938 cc. of the original crude oil). The chemical method of separating the paraffin wax used by the author in the tests referred to above is one which has been devised by him in conjunction with his colleague Mr. T. Stenhouse, and it is proposed to make this and the determination of bitumen the subject of a separate communication, which it is hoped to bring before the Institution when some final experimental work, now being carried out, has been completed.

The last column of Table I gives the results of the fractional distillation of crude petroleum oil obtained from the Hardstoft boring, which are of interest.

For obvious reasons up to the present time laboratory distillation-tests on crude petroleum oils have not been frequently required to be made in this country, but it is possible that in the future conditions may alter, and in any case it is considered that it is desirable to put on record what the author's experience has shown to be a convenient procedure, and it is hoped that discussion and criticism

may either confirm these methods or suggest useful modifications or alternatives.

Distillation-Tests of Motor-Spirit.

Owing to the limited supply of motor spirit now available, the designer of internal-combustion engines, the refiner of the spirit, and the consumer, are all alike interested in the successful employment of petrol possessing the largest possible proportion of the higher boiling-point fractions. Progress in this direction has been marked throughout the past 15 years. Since the commencement of the War, scarcity of supplies due to the increase in the use of spirit-consuming motors, and also advances in engine-design have caused an acceleration of this progress. One result of the desirability of using motor-spirits containing larger proportions of the higher boiling-point fractions has been the very natural endeavour of the refiner to get the credit for his product for every unit of low boiling-point constituents which it contains, and on this account he is desirous of specifying the use of those methods of laboratory distillation-tests for grading motor spirit which will best secure this end.

Partly as a result of this position the number of different methods of making fractional distillation tests on petrol is very large.

The principal methods can be conveniently exhibited in the diagrammatic form given on p. 30. From this it may be seen that the chief varieties are eight in number, using a straight tube as a dephlegmator, and eight identical methods using any form of bulbed or other dephlegmator or fractionating column.

Besides the main variations shown on this diagram, the methods may further be varied by changes in small details which may be introduced into any one of the sixteen main methods mentioned. These details are:—

(1) The method of taking the initial distillation-point as having been recorded by the thermometer when the first drop of condensed distillate falls (*a*) from the side vapour tube, (*b*) from the end of the condenser tube, or (*c*) from the thermometer bulb.

(2) The volume of spirit taken for test, namely, 100 cc., or 500 cc., etc.

(3) The dimensions and pattern of the condenser.

(4) The dimensions and pattern of the distillation flask.

(5) The dimensions and pattern of the fractionating column.

(6) The position of the thermometer bulb.

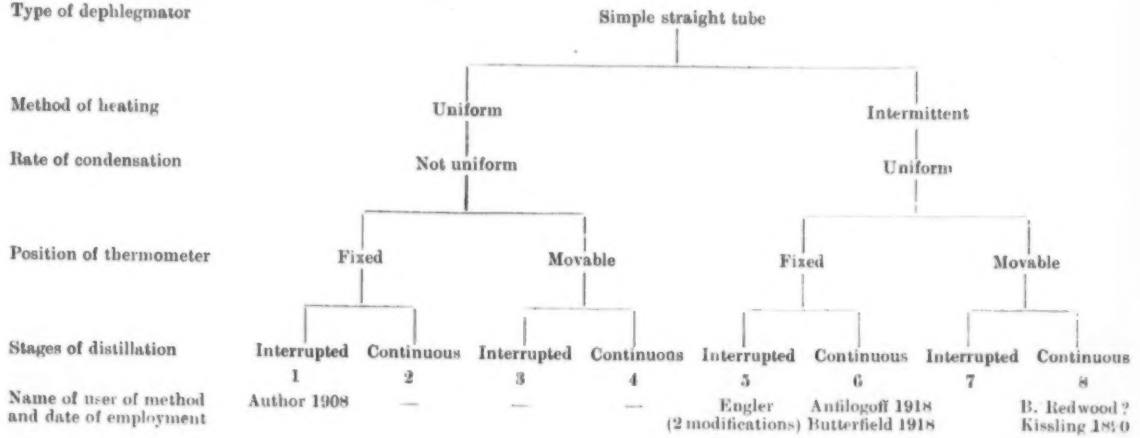
(7) The rate of distillation.

(8) The nature and arrangement of the heating apparatus.

(9) The method of determining the final distillation point.

DIAGRAMMATIC CHART OF CHIEF METHODS OF FRACTIONING PETROL USING A STRAIGHT-TUBE DEPHLEGMATOR.

(If a bulbed dephlegmator is used, a further series of eight methods otherwise similar to those given on this diagram is available.)



It is not desired to raise a discussion on all the details of the fractionation of motor-spirit at the present moment.

The many varieties of test in use are generally so well known that it appears useless to describe or criticise them further, until it is possible to do so under the auspices of some authoritative body charged with the duty of selecting and recommending a standard method of test for general adoption.

There is, however, one point in the distillation-tests of petrol, concerning which a remark must be made. As stated above under (1), the usual method of determining the initial distillation-point of motor-spirit is to record it as being the temperature at which the first drop of condensed spirit falls from the thermometer or other specified part of the distillation-apparatus.

The determinations of the distillation-point by this method, and by all analogous methods with which the author is acquainted, lead to somewhat discordant results in tests made on the same sample by different observers, and, indeed, even in repeat tests by the same observer. The precise meaning of the figure obtained by such tests, even if it were always found to be the same for the same sample of spirit, cannot be readily defined; it is certainly not the boiling-point of the liquid, and it can only be considered as a very rough measure of the pressure which the spirit is likely to set up when stored in hermetically-closed containers at any temperature. For these reasons the author prefers to ascertain the temperature at which the vapour-pressure of the spirit is equal to that of the atmosphere. This can be very readily done by means of what is known as the U-tube method of determining boiling-points. In order to make this test rapidly and conveniently, the author uses the following arrangement:—

A U-tube (A, Fig. 4) is closed at the shorter limb by means of a tap E, above which is a small cup B, having a capacity of about 4 cc. Below this cup is a graduation-mark C, on the tube a quarter of an inch below the top of the rounded end. To use the apparatus, dry clean mercury is poured into the open limb of the U-tube, the tap being open, until it stands at the mark H at the height of about two inches in each limb. The tube is then inclined so that the mercury fills the left-hand limb, and a small amount is allowed to flow up into the cup B; the tap is next closed, and about 3 cc. of the petrol to be tested is placed above the mercury in B. The tap E is then carefully opened until the mercury level just sinks to the line C, sucking in petrol after it. E is then again closed, and any residue of petrol in B is removed by filter-paper, and the U-tube is supported vertically by a clamp in a large beaker

of water, at such a depth that the water-level is about an inch above the top of B. The temperature of the water is then raised with constant stirring until the level of the mercury is the same in both limbs of the U-tube, the U-tube being so made that these are as close to one another as possible. When the level of the mercury is the same in both tubes the temperature of the water bath is the boiling-point of the petrol at the particular barometric pressure then existing.

A small correction ought to be made for the weight of the layer of petrol over the surface of the mercury in the left-hand limb of the U-tube, but as this is practically a constant for all petrols, if the same length of column of petrol is admitted for each experiment, that is to say, down to the mark at C, and is in any case very small, it may be ignored. As is well known in measuring boiling-points by this method, if the temperature of the bath is allowed to rise and fall several times, a corresponding number of repeat measurements of the boiling-point may be readily made on the same quantity of liquid, and concordant results can be easily obtained. The precise meaning of the figure obtained is that it is the temperature at which the vapour-pressure of the petrol is equal to the barometric pressure at the time of the experiment. If the liquid was a pure chemical compound, this would therefore be its boiling-point at that pressure, and even for a volatile liquid like petrol, which is a complex mixture of many constituents, the boiling-point of which mixture is difficult to define, it may be regarded as the nearest approach to this constant which can be made.

Determination of the Calorific value of Motor-Spirit by the Bomb Calorimeter.

It is considered that the best method for determining the calorific value of motor-spirit of high calorific value in a bomb calorimeter is as follows:—

The liquid is weighed out into a small stoppered glass tube,* or into a sealed glass bulb, such as are used in vapour-density determinations by Victor Meyer's method. The stoppered tube or sealed bulb containing the petrol is then placed on the platinum tray of the bomb in contact with a small weighed quantity (about 0·25 gram) of pure naphthalene which has been previously fused, and the ignition-wire adjusted so as to permit of the electric ignition of the naphthalene after the bomb has been closed. On firing,

* The glass must be thick enough to withstand the 30-atmosphere pressure of the oxygen in the closed bomb.

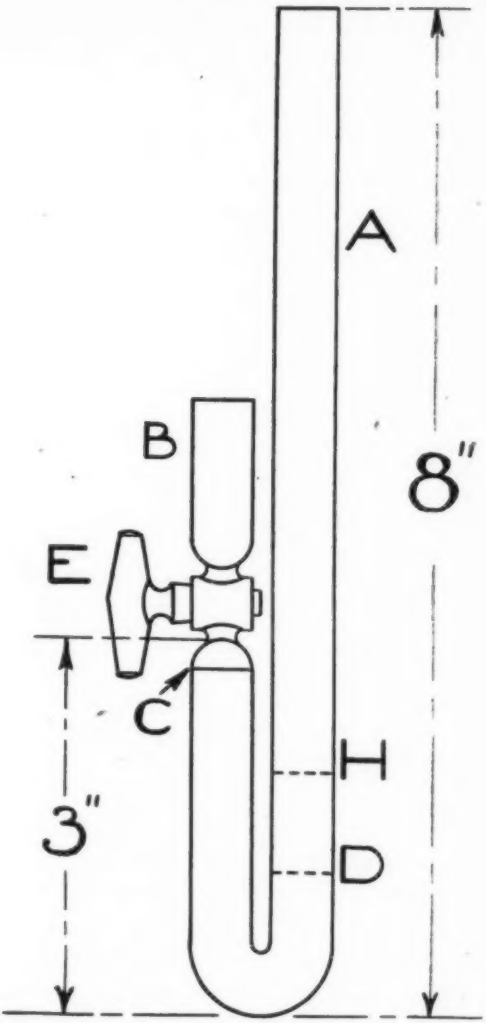


FIG. 4.—U-tube for Boiling-Point Determinations.

the heat liberated by the combustion of the naphthalene blows out the stopper of the glass tube, or bursts the bulb, and the spirit is thus liberated and ignited without any possibility of loss occurring, a possibility which, as far as the author is aware, is very real in all other methods of burning such volatile liquids in a bomb calorimeter in the interval between weighing out the petrol and closing the bomb.

It is not desirable to use a long narrow sealing-off tube if a sealed glass bulb is employed for this determination, for experience has shown that, after the explosion has taken place, traces of spirit may remain unburnt in the narrow end of such a tube. The combustion of petrol in a calorimeter bomb causes a much higher temperature to be set up than when the same weight of a heavier oil is dealt with, in consequence of the high calorific value of petrol, and the quickness with which it burns, its volatility causing it to mix rapidly with the oxygen. Thus the bomb is exposed to a correspondingly high pressure, and on account of the danger that this may damage or even burst the bomb, the weight of petrol taken must be less than that used for oils of lower calorific value and less volatility.

For heavier oils about one gram is usually taken for calorimetric tests, and if for petrol about 0·5 gram is used, with 0·25 gram of naphthalene, the percentage-error of test is somewhat raised, but it is less than 0·6 per cent., which is quite satisfactory for all practical purposes.

The "Water Values" and High and Low Calorific Values of Fuels.

If the measurement of the calorific value of a fuel is made it is always reported as the high or gross value, namely as the total number of units of heat evolved by the combustion of unit weight of the fuel in air. This calorimetric determination is a direct measurement, and as made by means of the bomb on solid fuels or heavy oil fuels, can be relied upon to possess an error of not more than 0·3 %. For volatile fuels such as motor-spirits the error of the high calorific value if determined as described above is as stated generally of the order of as much as 0·6 %.

When a fuel is to be used for engineering experimental tests of efficiency, either on boilers or internal-combustion engines, it has hitherto been the general practice to furnish a second calorific value, which has been known as the low or net value.

The precise definition of the high calorific value is clear and simple, but the definition of the low calorific value varies. In

France and Germany, the low value is calculated from the high by deducting from it the latent heat at 212° F. of the steam formed by any water in the products of combustion of the fuel, whilst in England the low value is usually obtained by making a further reduction from the low value as defined by Continental practice of the extra amount of heat given up in cooling the condensed water from 212° F. down to 60° F.

If either of these methods for calculating the low calorific values of fuels are adopted, it is usual to determine experimentally the percentage of ash (a) and the percentage of hydroscopic water (w) present in the fuel, and either to determine the percentage of hydrogen (h) in the water- and ash-free fuel by combustion, or to employ mean values for this last figure which have been obtained from series of tests made on fuels of a similar class.

Some of these values for hydrogen percentages for coals, heavy oil fuels and motor spirits of all classes are given in column *b* of Table II.

In Table II, the figures in columns *a* to *i* signify as follows:—

Under Results of tests made on fuels, *a* is the high calorific value in B.Th.U. per lb.; *b* the percentage of hydrogen in water- and ash-free fuel; *c* the percentage of water and ash in fuel. Under Correction for steam, *i.e.* the correction of the high calorific value of fuel as given in *a* for the latent heat of steam at 212° F. formed from the hydrogen in the water- and ash-free fuel as given in *b* calculated from the formula 0·8684 h (100–w–a), *d* is the correction in B.Th.U. per lb.; *e* the corrected calorific value in B.Th.U. per lb.; *f* the correction calculated as a percentage of *a*. Under Method of correction, *i.e.* the approximate method of correcting high calorific values of fuel as given in *a* for the latent heat of steam at 212° F. formed from the hydrogen in the water- and ash-free fuel as given in *b* by the subtraction of an arbitrary constant, *g* is the arbitrary constant in B.Th.U. per lb.; *h* the approximately correct calorific value in B.Th.U. per lb.; *i* the percentage of *h* to be added to or taken from *h* to make it the correct value.

The results of the author's tests are marked A; calculations from chemical formulæ, C; estimated probable round values, E; results L are from Landolt and Börnstein's Physikalisch-chemische Tabellen, ed. 1905, pp. 416, 418 and 425; P from Percy's Metallurgy, Vol. on Fuel, ed. 1875, pp. 303 to 348; R from Redwood's Treatise on Petroleum, ed. 1913, Vol. I., p. 238.

TABLE II.—High and Low Calorific Values of Fuels.

	Description of fuel.	Results of Tests made on fuels.			Correction for steam.			Method of correction.			
		a	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	
1	French non-caking coal (min.) ..	15,000 E	3·65 P	{water 5 % ash 2·0 % E}	294·7	14,705·0	1·96	396 (for coals)	14,604	+ 0·69 %	1
2	British caking coal (max.)	13,000 E	6·18 P	do.	499·1	12,501·0	3·84		12,604	− 0·82 %	2
3	Mean values for coal	14,000 E	4·91 P	do.	396·6	13,603·0	2·83		13,604	+ 0·00	3
4	American petroleum oil fuel (min.)	18,600 E	11·82 R	{water 0·5 % ash nil E}	1,021·0	17,579·0	5·49	1,111 for heavy oil fuels	17,489	+ 0·51 %	4
5	,, ,, ,, ,, (max.)	19,600 E	14·80 R	do.	1,278·0	18,322·0	6·52		18,489	− 0·90 %	5
6	Mean values for American oil fuels	19,100 E	13·31 R	do.	1,150·0	17,950·0	6·02		17,989	− 0·21 %	6
7	Petroleum oil fuel (American) ..	18,980 A	11·19 A	{water 0·482 % ash nil A}	966·8	18,013·0	5·09		17,869	+ 0·80 %	7
8	Gremontine white oil	20,050 A	14·48 A	nil	1,257·0	18,793·0	6·27		18,939	− 0·77 %	8
9	North country creosote . .. (containing 14 % naphthalene).	16,800 A	8·34 A	0·4 % A	721·4	16,078·0	4·29		15,689	+ 2·48 %	9
10	Olefines—propylene	21,120 L	14·28 C	nil	1,240·0	19,880·0	5·87	1,056 for motor spirits	20,064	− 0·92 %	10
11	iso-butylene	20,900 L	14·28 C	nil	1,240·0	19,660·0	5·93		19,844	− 0·93 %	11
12	iso-amylene	20,700 L	14·28 C	nil	1,240·0	19,460·0	5·98		19,644	− 0·93 %	12
13	Paraffins—pentane	21,170 L	16·66 C	nil	1,446·0	19,724·0	6·83		20,114	− 1·94 %	13
14	hexane	20,910 L	16·28 C	nil	1,414·0	19,496·0	6·76		19,854	− 1·80 %	14
15	heptane	20,741 L	16·00 C	nil	1,390·0	19,351·0	6·70		19,685	− 1·69 %	15
16	decane	20,095 C	15·49 C	nil	1,345·0	18,750·0	6·69		19,039	− 1·52 %	16
17	Ethyl ether	15,850 L	13·51 C	nil	1,173·0	14,677·0	7·39		14,794	− 0·79	17
18	,, alcohol (absolute)	12,750 L	13·04 C	nil	1,132·0	11,618·0	8·88		11,694	− 0·65 %	18
19	,, ,, (95 %)	12,112 L	13·04 C	{water 5·0 % ash nil C}	1,075·4	11,037·0	8·88		11,056	− 0·17 %	19
20	Acetone	13,240 L	10·34 C	nil	897·8	12,342·0	6·78		12,184	− 1·29 %	20
21	Benzene (90 %), toluene (10 %) ..	18,465 L	7·79 C	nil	677·4	17,787·6	3·67		17,409	+ 2·17 %	21
22	Benzene	18,440 L	7·69 C	nil	667·7	17,772·3	3·62		17,584	+ 2·23 %	22
23	Naphthalene	17,462 L	6·25 C	nil	542·6	16,919·2	3·10		16,406	+ 3·12 %	23

In order to calculate the low calorific value of the fuel from these figures in accordance with the Continental practice, the following expression must be used:—

$$(C)\ \left\{\begin{matrix}\text{Low Value in}\\ \text{B.Th.U. per lb.}\end{matrix}\right\} = \left\{\begin{matrix}\text{High Value in}\\ \text{B.Th.U. per lb.}\end{matrix}\right\} - 0{\cdot}8684\ h\ (100 - w - a)$$

where h = the percentage of hydrogen in the water- and ash-free fuel,

w = the percentage of hygroscopic water in the fuel as burnt,

a = the percentage of ash in the fuel as burnt.

If the English practice is followed of deducting the further amount of heat given up in cooling the water from the condensed steam from 212° F. down to 60° F., the expression becomes:—

$$(E)\ \left\{\begin{matrix}\text{Low Value in}\\ \text{B.Th.U. per lb.}\end{matrix}\right\} = \left\{\begin{matrix}\text{High Value in}\\ \text{B.Th.U. per lb.}\end{matrix}\right\} - 1{\cdot}005\ h\ (100 - w - a)$$

The expression 0·8684 h (100 − w − a) used in equation C. above, is the number of B.Th.U. per pound of fuel burnt, which, in the form of the latent heat of steam derived from the water formed from the combustion of the combined hydrogen in the fuel, can escape with the flue gases of a boiler furnace or in the exhaust of an internal-combustion engine, without being detected by the thermometer, if, as is practically invariably the case, these gases are at a temperature of over 212° F. It is considered that the Continental method of calculating the so-called low calorific value of a fuel from its high value is more satisfactory than the English, hat is provided always that the term low or net calorific value is to be retained at all.

In column *d* of Table II, the values of the corrections 0·8684 h (100 − w − a) are given for a series of fuels, and in column *e* the corresponding corrected values from the low calorific values as understood on the Continent are stated: whilst column *f* gives the values of the percentages of heat which will escape unrecorded in the exhaust gases in boiler and engine trials, if this correction is not taken into consideration.

As shown under method, &c., columns *g*, *h* and *i* of Table II, very close approximations to the true low calorific values (Continental style) can be obtained by subtracting arbitrary constants from the high calorific values of coals and heavy petroleum oil fuels, but for creosotes and motor spirits this approximate method of calculating is not satisfactory. If the nature of the constituents of a motor-spirit is known, that is to say, as to whether it consists of petrol, acetone, benzol, etc., or of mixtures of these constituents, with or without the addition of naphthalene, and if also the proportions in which these constituents are present are approximately

known, it is comparatively easy to calculate the low from the high calorific value by means of a constant. As a matter of fact, however, the necessary knowledge of the constitution of motor spirit for this purpose cannot in general be obtained with any accuracy, unless a somewhat elaborate examination is carried out, and the difficulty of the necessary tests, and the time occupied is greater than that required for a direct determination of the combined hydrogen present by means of a combustion, from which by the use of the above equations C and E, the low values may be accurately determined.

In the above remarks on the determination of low calorific values, no account has been taken of the further amount of latent heat which escapes in flue gases at temperatures above 212° F., in the vapour of the water which is present in the fuel as hygroscopic moisture. When this is taken into account, it necessitates a further subtraction of heat units from the low calorific values as determined by the equations C and E.

The equations corrected for this further cause of heat loss then become:—

$$C' \left\{ \begin{matrix} \text{Low Value} \\ \text{in B.Th.U.} \\ \text{per lb.} \end{matrix} \right\} = \left\{ \begin{matrix} \text{High Value} \\ \text{in B.Th.U.} \\ \text{per lb.} \end{matrix} \right\} - 0{\cdot}8684\,h\,(100 - w - a) - 9{\cdot}648\,w.$$

$$E' \left\{ \begin{matrix} \text{Low Value} \\ \text{in B.Th.U.} \\ \text{per lb.} \end{matrix} \right\} = \left\{ \begin{matrix} \text{High Value} \\ \text{in B.Th.U.} \\ \text{per lb.} \end{matrix} \right\} - 1{\cdot}005\,h\,(100 - w - a) - 11{\cdot}168\,w.$$

The additional corrections of 9·648 w B.Th.U. and 11·168 w B.Th.U. made for the water present as hygroscopic moisture in fuels are so small for the amount of water usually found in fuels that they are unimportant when compared with that required for the combined hydrogen present in the water- and ash-free fuel. Thus, even for such a small gross calorific value as that possessed by 95 % ethyl alcohol, namely, 12112 B.Th.U. per lb., the whole of the reduction of available heat due to the 5 % of water present is only 48·24 B.Th.U., which is less than 0·4 % of the high value.

The water formed from the combustion of the combined hydrogen in a ton of fuel varies from a minimum of 0·3285 ton for a French non-caking coal (Table II, line 1) up to 1·494 tons for pentane (*Ibid*, line 18). Therefore, in burning a ton of fuel, it must evaporate from 0·3285 to 1·494 tons of water derived from the combined hydrogen which the fuel contains. The further amount of water which is present in the hygroscopic form in one ton of the fuel, and which must also be evaporated, only amounts to from about 0·02 ton for 2 % to 0·05 ton for 5 % of hygroscopic

moisture: hence the determination of the moisture in a fuel, in so far as it affects its low calorific value, is much less important than the correct measurement of the amount of combined hydrogen present.

As stated above, if the use of the low or net calorific value is to be retained in the future, the author considers that it is desirable that, for the sake of uniformity, it should be calculated in one form only, and that the form adopted by Continental writers is the most satisfactory for use as being less ambiguous.

It is, however, believed that it is most desirable that the use of the expression of low or net calorific value should be abolished in the future, and that in reporting upon all fuels from the point of view of their heating capabilities only two directly-ascertained results should be given. These are (1) the High Calorific Value determined by the bomb calorimeter as now commonly reported, and (2) what the author designates as the "*Water Value*" of the fuel, that is the total number of units of weight of water obtained from the combustion of unit weight of the fuel, by which is meant the sum of the water derived from the oxidation of the combined hydrogen in unit weight of the fuel, together with the hygroscopic water contained in that weight. From these two values, the calorific value and the water value, any form of the low or net calorific value can be at once determined in any unit desired by the most simple calculation. If this procedure is generally adopted, it will remove any possibility of the ambiguity which at present exists concerning low calorific values. The engineer will be provided with the calorific value of the fuel (high value) and the "water value," both of which are direct determinations, and these will supply him with all the information necessary to make any desired calculation to suit the special conditions of the thermal tests which he is carrying out.

The determination of the water value of a fuel by combustion may either be carried out in an ordinary tube combustion-furnace or, as the author finds, more conveniently and more rapidly by means of that form of the bomb calorimeter which is provided with entry and exit tubes, such as the bomb known as the Mahler-Kröcker Bomb.

In order to carry out the determination of the calorific value and of the water value in the same operation the following procedure is adopted:—

The fuel is weighed out (one gram for solid fuel or heavy oil fuel, and for motor-spirit about 0·5 gram in a glass-stoppered tube, together with 0·25 gram of naphthalene as described on page 32

above). After placing the weighed fuel in the bomb and closing it, about a litre of oxygen gas is run through in a gentle stream, in order to remove the nitrogen present, and thus to reduce the formation of nitric acid to a minimum. The exit tube is then closed, and oxygen is admitted until the pressure rises to 30 atmospheres, when the determination of the calorific value is completed in the ordinary manner.

The exit-tube of the bomb is next connected to an absorption train, consisting of the following tubes connected in series: first, a small glass U-tube immersed in broken ice, where most of the water is deposited; second, a five-inch U-tube filled with pumice saturated with strong sulphuric acid in which the remainder of the water is absorbed; thirdly, three Mohr's potash bulbs connected in series, in which the CO_2 is absorbed; and lastly, a calcium-chloride guard tube. The exit tube of the bomb is then gradually opened, and the gas is run through the absorption-tubes at the rate of about two or three bubbles per second. When the gas ceases to flow, the entry tube of the bomb, which has been connected to the usual air purifying train to remove CO_2 and moisture, is opened, and air is sucked through, whilst the bomb is heated up to a temperature of about 250° F. by means of an air bath. The time required to reduce the pressure of gas in the bomb to that of the air is from 2 to 3 hours, and the time necessary for the aspiration of sufficient dry air through the bomb to remove all water and CO_2 is a further period of about 60 to 90 minutes. The whole operation of determining both the calorific value and the water value of a fuel, and also, if desired, the amount of carbon and of sulphur contained by it, is therefore about four hours, and very concordant results can be obtained even with the most volatile motor-spirits.

As regards the percentage-error involved in this method for determining the water-value of a fuel, it is observed that if 0·5 gram of motor-spirit is taken for test, and if this spirit contains the very low percentage of hydrogen present in benzene, namely, 7·69 per cent., the weight of water formed is 0·34605 grams, and as it is easy to obtain concordant weighing results to within 2 mgr. of the water formed, the maximum error cannot be greater than 0·29 per cent. on the water value of the fuel. The possible percentage-error if one gram of the fuel is taken, as can be done with solid fuels and heavy oil fuels, is correspondingly less.

There are, however, sources of special error in this method of combustion, which, if recognised, can be reduced to a minimum by suitable precautions. These are:—

First, the possibility of the formation of nitric acid during the

explosion in the bomb, which acid will later be absorbed with the water, and cause this result to be too high. This can be avoided, firstly, by getting rid of the greater quantity of the nitrogen in the bomb by driving out the air by a current of oxygen before proceeding to determine the calorific value, whilst secondly, the water which collects in the U-tube cooled in ice, which forms the first item in the absorption train, can, after it has been weighed, be titrated in order to estimate any acid it contains, for which a corresponding deduction from the weight of the water can afterwards be made. In this connection it is observed that the error due to the formation of nitric acid has been found in practice to be negligibly small when tests are carried out as described above, amounting in two determinations to 5·2 and 3·7 B.Th.U per lb. of fuel respectively.

The second source of error is due to the sulphuric acid formed by the combustion of any sulphur in the fuel. This acid will retain a certain proportion of water in the bomb when it is heated up to 250° F., whilst the stream of air is being passed through it.

If it is assumed that the sulphuric acid left in the bomb after drying out with air at 250° F. contains 50 per cent. by weight of H^2SO_4 (an acid whose boiling-point Lunge gives as 260° F.) this correction amounts to adding 0·0362s units to the uncorrected water value of the fuel, where "s" is the percentage of sulphur which is present in the fuel. The correction is therefore usually small for heavy oil fuels, and is negligibly small for motor spirits and kerosenes. For example, in oil fuel containing 1·0 per cent. of sulphur, and whose calorific value is even as low as 15,000 B.Th.U. per lb., the total correction for water retained by sulphuric acid is only 34·9 B.Th.U., or about 0·23 per cent. of the calorific value. For coals and similar solid fuels, where both sulphuric acid and water may be retained by the ash, the correction for water retained in the bomb cannot be made with such accuracy as with oils.

A knowledge of the calorific value of a fuel in conjunction with its water value enables the efficiencies of boilers, internal-combustion engines or other fuel-using apparatus to be correctly determined.

Such determinations are, however, comparatively seldom required, and the chief use of these constants in ordinary practice is either to enable a series of fuels of different character to be arranged in their order of merit for use for some specific heating purposes, or to make it possible to ascertain whether a given fuel does or does not comply sufficiently with the limits laid down by some specification.

The high calorific value by itself is indeed useful for such purposes, as it gives a figure of merit for a material, which is of the nature of a specific gravity, refractive index or viscosity-determination, and thus assists in enabling a delivery to be accepted as satisfactory, or rejected as not being up to specification.

The Determination of the Amount of Water in Oils, Tars, etc.

Assuming that the bulk sample of oil as received, really correctly represents the contents of the steamer or land-storage tank from which it has been drawn, and that the procedure of preparing the sample for test from the bulk sample, as described on page 11, has been carefully followed, the next step is to decide upon the best method for ascertaining the actual amount of water in the representative laboratory sample thus prepared.

It is believed that up to as late as 1914 the method of determining water in oil fuels, as laid down by the New York Produce Exchange consisted in mixing the oil to be tested with an equal volume of kerosene or petrol, and then submitting this mixture to a high centrifugal force in specially-constructed glass vessels used in a centrifuge run at a specified speed.

This method has not been found satisfactory at Portsmouth, owing to the difficulty in obtaining glass vessels which were sufficiently strong to withstand the necessary strains. No doubt, a satisfactory centrifuge and glass vessel could be designed, and are possibly now on the market, but the author has not been able to obtain them, and during the war he has also been unable to procure the official description of the American method of test. The breakages in the centrifuge of the glasses with which experiments have been made by the author have apparently been due to the vibration caused by want of balance of the centrifuge, and also to the high pressures produced by the speed of rotation being too severe for the thin glass vessels supplied, the effect of the pressure being accentuated by the unsatisfactory character of the seatings provided to support the glasses. It has been stated that the specification for the test for water by the New York Produce Exchange has recently been altered, but as to whether this information is correct or not is not definitely known.

In any case it may be of interest to put on record the method of determining moisture in all oils, which has been used by the author for many years past.

The representative sample of oil, tar or creosote, which has been as far as possible separated from the visible water and emulsion as

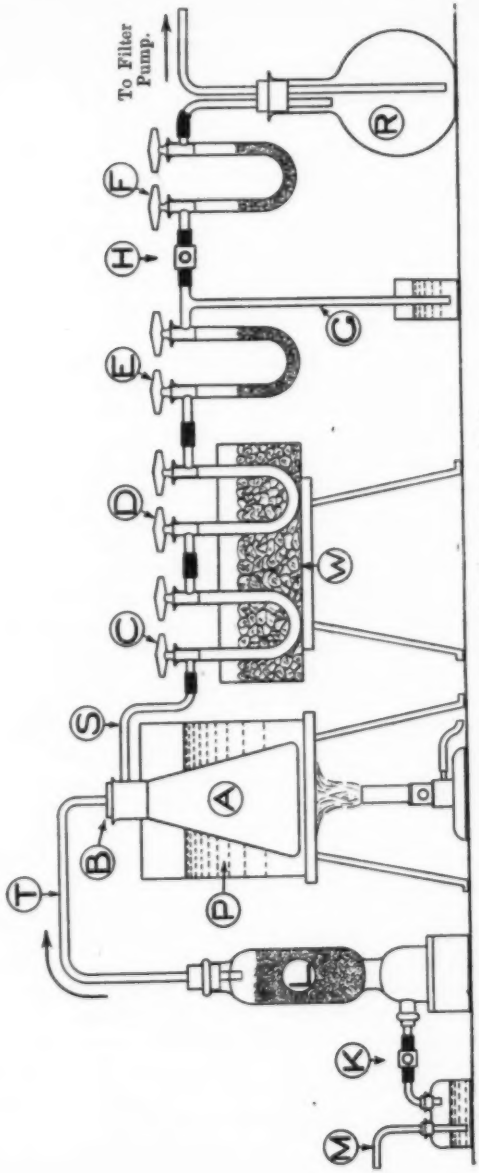


Fig. 5.—Apparatus for Water-Determination.

described on pp. 11, 12, is well shaken up, and 200 grams are weighed into a conical copper-sheet flask A, fig. 5. This has a capacity of about 800 cc., is fitted with side tubes, and is closed by means of an india-rubber bung B through which a glass leading-in tube passes to the bottom of the flask, its lower end being constricted to a fine orifice. The flask is immersed in a water-bath P. The side tube of the flask is connected to an absorption train which consists of the following items: Firstly, two small glass-stoppered U-tubes C and D, which are connected in series, and are surrounded with a freezing mixture of hydrochloric acid and ice, W. Secondly, two U-tubes E, F, containing calcium chloride. The last calcium chloride tube F serves as a guard tube to prevent any water-vapour from the filter pump gaining access to the U-tubes C, D and E. The outer limb of the guard tube is connected through a flask R to the filter pump. Between E and F a mercury pressure tube G is connected.

The tube T, leading into the copper flask A, is connected to a calcium-chloride tower L, and this in turn is connected to a small tell-tale bottle M, which contains heavy mineral oil, and serves to indicate the speed at which air is passing. Screw compression clips on rubber junction-pieces are situated at K and H.

When the apparatus has been connected up, the screw clip at K is closed, and the filter pump is started. When the mercury ceases to rise in the gauge tube G, generally to a height of about 20 inches, the screw clip at H is also closed, and the mercury column in G is observed, in order to ascertain as to whether any decrease in its height takes place, indicating a leak in the apparatus. If a leak is indicated, it must be traced and rectified. When this has been done, and the apparatus is air-tight, air is sucked through the system at the rate of about three bubbles per second, the rate being regulated by the screw clip at K. The water-bath is then raised to the temperature of nearly 212° F. With tars or creosotes containing large quantities of water it is desirable not to distil under a vacuum of 20 inches of mercury until after about 30 minutes. In the case of dry oils, containing less than ·5 % of water, it will all have been removed after 40 minutes and condensed in the U-tubes C, D and E. The clip at H is then closed, and when the air has ceased to bubble through the tell-tale M, the taps on the U-tubes are closed, and the tubes are removed, dried on the outside and weighed. If, as may occasionally be the case, any oil is condensed with the water in the U-tubes C and D, it can be seen, and may be separated and weighed, if it is present in a large enough amount seriously to affect the accuracy of the

determination. This separation is most conveniently carried out by treating the mixed oil and water with sufficient anhydrous copper-sulphate to combine with all the water, and the oil is then extracted from the copper-sulphate crystals by means of special dry light petrol.

This method of determining water in oils possesses a high accuracy, for a weight of 20 mgr. of water weighed represents 0·01 per cent. of water in the oil. The determination of the water in a dry oil occupies a period of about 35 minutes, but for very wet oils, containing say 6 per cent. of water, it may require a period of as much as six hours.

When water is present in tars, and also to some extent in creosotes, trouble is not infrequently experienced in driving it off by means of bubbling a stream of dry air through it whilst heated to 212° F., for great frothing may occur. This frothing appears to be due to the fact that the bubbles of water in these liquids are sometimes very small, and therefore, as the extra pressure, above that of the external atmosphere, to which the water in the bubbles is exposed from the surface tension of the film enclosing them, is inversely proportional to their diameter, it may have a considerable magnitude, and prevent them from expanding into steam until the temperature is well above that necessary to produce a vapour-pressure equal to that of the external atmosphere. When, however, the temperature finally rises sufficiently high to cause the bubbles to expand, then increase in diameter at once reduces the pressure due to surface tension of the liquid envelopes of the bubbles, and hence their condition of equilibrium is unstable, and they expand very rapidly, causing the phenomenon known as frothing, which is especially marked if the number of bubbles of water present in the tar is great, and their diameter is small.

A method of avoiding trouble due to frothing, which has been found to be very satisfactory, is to mix the 200 grams of the tar, creosote or oil taken for test, as described above, with about an equal weight of sand or of pieces of dried pumice, fire-brick or other porous material of about $\frac{1}{4}$ inch diameter. The dried porous material should be heated to a dull-red heat, and then stored, whilst still hot, in a stoppered bottle until required for use.

It is very convenient to employ a routine method for determining water in all oil fuels, creosotes or tars, for it is not possible to be certain by inspection that a given sample will not cause trouble due to frothing. The author therefore makes it the practice to mix dried pumice with all such samples when tested for water, and it is found that this precaution avoids the delay required

for making the repeat tests which are sometimes found to be necessary if frothing takes place. It is chiefly because pumice, sand or fire-brick are difficult to clean out of glass flasks without breakage that a conical copper sheet flask is recommended for use in these determinations.

If more than about 5 cc. of water is collected in the glass U-tubes from 200 grams of oil, the test should be repeated on a conveniently smaller quantity of the oil.

Although the above method of determining water in oils is applicable to all classes of oils or tars, and for this reason is very useful for routine laboratory work, it is remarked that the special method of distilling tar after admixture with dry coal-tar naphtha, as proposed by Allen and Jacobs (*J. Soc. Chem. Ind.*, 1912, p. 862) and as modified by S. R. Church and others, is stated to yield excellent results and to prevent any tendency to froth. For the description of this and other similar methods for determining waters in tars, *vide* "Coal Tar Distillation," by A. R. Warnes, Ed. 2, pp. 225–226, 1917.

The determination of water in ordinary petrol, shale spirit or cracked spirit is not of much practical importance at present, for it can only exist in such liquids in very minute quantities without being visible, when its volume can be measured directly. Any turbidity caused on cooling such spirit down below, say, 25° F. (− 4° C.) may usually be safely attributed to the presence of minute traces of moisture, but in such amounts the water is not of practical importance unless the spirit is required for some special purpose.

If, however, as appears possible, the use of composite motor-spirits containing alcohol and acetone becomes general, it will render the determination of water in these mixtures a matter of practical importance.

The recent report on the Inter-Departmental Committee on Alcohol for Power and Traction Purposes, Section 13, makes the recommendation that in all admixtures of alcohol, benzol, ether, petrol or the like sold as power-alcohol, the ratio of water to alcohol after admixture shall not exceed one part by volume of water to nine parts by volume of alcohol. If and when this recommendation is put into force, it will be necessary to possess a means of rapidly and accurately determining the water present in such spirit. It will be extremely difficult to do this by means of distillation, on account of the volatility of the constituents other than water, and on this account it appears to the author that the problem will have to be dealt with by using a method of salting out the water present by some process similar to that proposed by C. J.

Haines and J. W. Marden (*J. Ind. Eng. Chem.*, 1917, pp. 1126–1127, abstracted in *J. Soc. Chem. Ind.*, February, 1918, p. 108A), and also subsequently by N. C. Nag and P. Lal, *J. Soc. Chem. Ind.*, 1918, p. 290T. Experiments are now being carried out by the author on these lines, and the use of anhydrous copper-sulphate as a water absorbent is also being tested.

In the event of methods for determining the amount of water present in motor-spirits on these lines being found to possess insufficient accuracy, it is possible that the calorific value of the spirit, taken in connection with its "water value" may provide a satisfactory basis for fixing the limits of hygroscopic water present in such fuels.

The Free Acidity of Oil Fuels.

Tars and also creosotes are usually alkaline in reaction, owing to the presence of traces of ammonia. Petroleum oil fuels may possibly possess an acidity soluble in water owing to refinery products containing traces of sulphuric acid having become intentionally or accidentally added to the oil. Naturally as petroleum oil fuels are not refined, the presence of the traces of sulphuric acid sometimes found in refined petroleum oils from incomplete washing are not to be expected. It has, however, been stated that in tar distilleries anything on the works which is not wanted is creosote: how far such a definition of creosote may be pushed the author is not in a position to state, but clearly (as human nature is very much the same everywhere) it may be possible in a petroleum oil refinery for products which have been in contact with sulphuric acid to become absorbed into the oil fuel. In any case such free acidity, whatever its origin, is particularly objectionable on account of the corrosion it may cause in storage-tanks, pipe-lines, etc., and in the author's opinion it must always be looked for, although it has been his experience that out of a good many thousand samples of oil fuel of every description which he has examined, none has been found to possess free acid soluble in water.

The amount of free acidity in oil fuels, due to the presence of organic acids dissolved in the oil which are practically insoluble in water, varies a good deal, as is shown from the following figures obtained from tests made on 86 samples of different origin taken at random:—

Description of Oil.	Number of Samples tested.	Free organic acids as Oleic Acid. Normal samples.	Exceptional samples.
American Oil Fuel.....	27	0·052% to 0·420%	One containing 1·984%.
Trinidad Oil Fuel	7	0·730% to 1·52%	One containing 0·337%.
Texas Oil Fuel	9	0·118% to 0·725%	One containing 1·984%.
Mexican Oil Ful	3	0·067% to 0·100%	None.
Persian Oil Fuel	25	0·034% to 0·118%	None.
Borneo Oil Fuel.....	15	0·077% to 0·219%	None.

The erratically high results for free organic acid in single samples of American and Texas oils shown above must not, however, be regarded as being specially characteristic of these oils, for they probably may occur in oil fuels from other sources. Owing to the fact that time has prevented the records of analysis of a larger number of oils being collected, the results quoted must be regarded as somewhat accidental in character.

It is the author's custom to report both the inorganic acid soluble in water, and also the organic acid soluble in alcohol in terms of oleic acid. The advantage of this procedure is that as it is the usual custom to return the free organic acid present in lubricating-oil as oleic acid, it makes all results directly comparable with one another, whilst as the combining weight of oleic acid is nearly six times that of sulphuric acid, the percentage of acid returned as present as oleic is correspondingly high, and this tends to secure for the return the attention which it deserves.

The precise nature of the organic acids present in petroleum fuel oils is not known to the author, nor is it known as to what action, if any, they possess in causing the corrosion of iron or other metals in contact with them.

As stated above the free acidity to water of all oil fuels examined by the author has been *nil*, but if in any case such free acidity is observed, it is considered that its nature should be ascertained, and its presence specially remarked upon in any report dealing with the oil.

It is necessary to determine separately the free acidity of oil soluble in water and that insoluble in water but soluble in alcohol.

The methods used by the author for determining free acidities are as follows:—

(1) Free acidity to alcohol:—

If the oil is not very dark or thick, an amount of 50 grams is

weighed into a 10-ounce bottle, and to this 100 cc. of neutralised alcohol, together with a few drops of neutralised phenolphthalein solution, is added, and the mixture is titrated with a deci-normal solution of caustic soda, in quantities of not more than one-tenth of a cc. at a time, the mixture being well shaken after each addition, and then allowed to settle. The additions are continued until the separated alcohol just turns pink.

In the case of thick or dark-coloured oils, a quantity of 50 grams of the sample is weighed into a 300 cc. glass flask, and an amount of 150 cc. of neutral alcohol added. The flask, covered with a watch glass, is heated on the top of a water-bath, but the alcohol should not quite boil. When hot, the mixture is well shaken up, and it will be found that the alcohol will then rapidly separate from the oil, and will be sufficiently clear within from 10 to 15 minutes to permit of its titration with deci-normal soda, using phenolphthalein as an indicator. The oil is again well shaken up after the addition of alkali, and once more heated and allowed to settle, and the titration repeated until a permanent pink colour appears. The whole operation may take as much as 6 or 7 hours to complete, but if it is attempted to carry it out in the cold, the periods required to separate the oil and alcohol will cause the test to take at least three times as long.

(2) Free acidity to water:—

A quantity of 50 grams of the oil is weighed into a 250 cc. separating funnel. A quantity of 100 cc. of distilled water, previously made just neutral, to methyl orange is added. The mixture is then thoroughly shaken, allowed to settle and the separated water as far as possible run off into a beaker. This operation is again repeated with two further successive quantities of 50 cc. of neutralised water. The total washings are then mixed, and titrated with deci-normal caustic soda, using methyl orange as indicator. In the case of some very thick oils, separation of the water from the oil in the above operations is found to be inconveniently slow and tedious, and in such cases hot water is used, and if necessary, the whole separating funnel is heated by immersing in a hot-water bath.

The Effect of Moisture in Mineral Oils upon their Flash-points.

All mineral oils, whatever their origin, contain certain definite proportions of moisture. These proportions vary a good deal with the class of oil, as is shown by the following tabular statement of approximate limits:—

Description of Oil.	Grams of Water per 100 cc. of Wet Oil.
Transformer oils	0·005 to 0·017
Mineral lubricating oils ...	0·01 and over
Distilled petroleum fuel oils ...	0·01 and over
Undistilled petroleum fuel oils	Usually 0·2 to 0·6 and over
Creosote oils	Frequently much above 0·5

It is further evident that, owing to the conditions of sea transit, the amount of water in fuel oils carried in bulk may often accidentally be much higher than the figures quoted above, and in certain samples of oils taken from near the bottoms of tanks of oils possessing a gravity of less than unity very large amounts of water may be present, owing to segregation, whilst in creosotes or heavy tar-oils samples from the tops of tanks may show the same peculiarity.

It is well recognised that the presence of moisture in an oil will cause its flash-point to vary in some way, but, as far as the author is aware, the nature of this variation and its amount is not known, or has not been recorded, whilst the connection between varying amounts of water present in a given oil and its flash-point, and the effect of the presence of a given quantity of water in oils of higher and lower viscosity and flash-point (observed in the dry state), and also in oils of different chemical character such as petroleum oils, creosote oils, shale-oils, etc., has not it is believed been investigated hitherto.

In the directions for use of the Pensky-Martens flash-point apparatus as usually issued with the instrument the following caution as to the effect of water upon the readings is given :—

> " It is also necessary to keep the oil-cup perfectly dry, as traces of moisture interfere with the production of the flash. Care must therefore be taken that the oil is free from water before testing." Baird and Tatlock's Catalogue, 1914 edition, p. 909.

In his " Treatise on Petroleum," 1913 edition, vol. ii, p. 268, Sir Boverton Redwood remarks concerning the Pensky-Martens tester for lubricating oils :—

> " In employing this or any other form of close-test instrument, in the testing of lubricating oils " (and presumably oil fuels) " it is important that the samples should be free from water, as the presence of aqueous vapour in the upper part of the cup prevents the occurrence of the flash."

The experiments which the author has carried out on this question appear to show that the most marked effect due to the presence of water in an oil having a flash-point above 150° F. is that it causes much greater variations in the results of a series of repeat flash-point

tests made by a series of independent observers all testing the same sample of oil than are found when a similar dry oil is tested in the same way. If the oil containing water has a flash-point (when dry) of above 212° F., the variations between the results of repeat flash-point tests obtained by different observers are more marked than is the case if the oil has a flash-point less than 212° F. The variation between repeat flash-point tests on an oil containing water is in fact generally less the lower the flash-point of the oil.

In the directions for preparing and using apparatus for testing petroleum oil in temperate climates, drawn up by Sir Frederick Abel in 1887, no statement is made which indicates the variations in results of repeat tests made by the same or different observers which may be considered as being reasonable when employing this apparatus, but in the testing schedule of the Indian Petroleum Act of 1899 (as modified up to 1903), which relates only to oils flashing below 95° F., it is stated that if no greater difference than 2° F. exists between any two of three consecutive and separately-made determinations the mean of the three may be taken.

The limit of accuracy of test laid down in the directions for use of the Abel-Pensky instrument in Germany, which is only used for oils flashing below about 100° F., is that if the results of three repeat tests do not differ by more than 1·5° C. (i.e. 2·7° F.) the mean may be taken as correct.

The author is not aware that any similar permissible limits of variation in check-tests made with the Pensky-Martens tester or any other flash-point tester for oils possessing a flash-point above say 120° F. have ever been stated.

The results obtained in the author's laboratory for the higher flash-point oils using the Pensky-Martens tester indicate that considerably greater variations than the 2° F. or 2·7° F. limits referred to above are generally obtained on both dry and wet oils.

On Table III are recorded the results of a series of six to eight repeat flash-point determinations, each made by independent observers on each of nine samples of wet oils, and also on each of seven samples of the same oils dried by passing a current of air through the heated oil by the method described on pp. 42 to 46. It is considered that these results indicate, firstly that the permissible limit of variation of repeat flash-point tests made on dry oils is certainly greater for oils flashing above 150° F. than the 2·0° F. to 2·7° F. laid down for oils having flash-points say under 120° F. But in no case did the flash-point determinations on a given dry oil show greater variations than 8° F. on a total of six repeat tests.

Secondly, the variations in the results of six to eight repeat

TABLE III.—Tabular statement of repeat determinations of the flash points of oils by

Description of oil ..	—	Creosote oil.		Creosote oil.		Texas oil fuel.		Creosote oil.	
Mark on sample ..	—	196 A/18.		2,872 A/18.		659 A/19.		507 A/18	
Water present on wet oil as received in grams per 100 cc.	—	7·0 per cent.		2·75 per cent.		1·5 per cent.		0·42 per cent.	
	Ref. No.	O.F.P.	V.	O.F.P.	V.	O.F.P.	V.	O.F.P.	V.
Results of repeat flash point determinations made by independent observers on wet oils as received.	1	194°	+ 10°	189°	− 6·5°	241°	+ 5·5°	174°	+ 2·5°
	2	171°	− 13°	194°	− 1·5°	223°	− 12·5°	171°	− 0·5°
	3	190°	+ 6°	199°	+ 3·5°	241°	+ 5·5°	167°	− 4·5°
	4	196°	+ 12°	198°	+ 2·5°	228°	− 7·5°	171°	− 0·5°
	5	189°	+ 5°	199°	+ 3·5°	232°	− 3·5°	174°	+ 2·5°
	6	185°	+ 1°	194°	− 1·5°	248°	+ 12·5°	172°	+ 0·5°
	7	174°	− 10°	—	—	—	—	172°	+ 0·5°
	8	172°	− 12°	—	—	—	—	171°	− 0·5°
Range of variations of observations.	—	—	25·0°	—	10·0°	—	25·0°	—	7·0°
Arithmetic mean of observations.	—	184°	—	195·5°	—	235·5°	—	171·5°	—
Results of repeat flash point determinations made by independent observers on the same oils dried by distillation.	9	—	—	194°	− 4°	235°	− 2°	172°	+ 2·5°
	10	—	—	198°	nil	234°	− 3°	171°	+ 1·5°
	11	—	—	201°	+ 3°	239°	+ 2°	169°	− 0·5°
	12	—	—	201°	+ 3°	241°	+ 4°	169°	− 0·5°
	13	—	—	198°	nil	237°	nil	169°	− 0·5°
	14	—	—	196°	− 2°	235°	− 2°	169°	− 0·5°
	15	—	—	—	—	—	—	169°	− 0·5°
Range of variations of observations.	—	—	—	—	7·0°	—	7·0°	—	3·0°
Arithmetic mean of observations.	—	—	—	198°	—	237°	—	169·7°	—

NOTE.—O.F.P. signifies observed flash point. V. signifies variation

the Pensky-Martens tester in the wet and dry state. All in Fahrenheit degrees.

Creosote oil. 883 A/19. 0·40 per cent.		Mineral lighting oil. 819 V/19. Under 0·10 per cent.		Light shale-oil. 318 V/19. Under 0·05 per cent.		Scotch shale-oil. 300 V/19. Under 0·05 per cent.		Turbine mineral lubricating oil. 1,514 T/14. 0·011 per cent.		
O.F.P.	V.	O.F.P.	V.	O.F.P.	V.	O.F.P.	V.	O.F.P.	V.	Ref. No.
192°	− 2·0°	136°	− 1·5°	160°	+ 3·0°	266°	− 5·5°	392°	− 3·0°	1
194°	nil	139°	+ 1·5°	156°	− 1·0°	270°	− 1·5°	399°	+ 4·0°	2
199°	+ 5·0°	136°	− 1·5°	154°	− 3·0°	277°	+ 5·5°	396°	+ 1·0°	3
194°	nil	138°	+ 0·5°	158°	+ 1·0°	271°	− 0·5°	390°	− 5·0°	4
196°	+ 2·0°	137°	− 0·5°	154°	− 3·0°	273°	+ 1·5°	401°	+ 6·0°	5
189°	− 5·0°	138°	+ 0·5°	156°	− 1·0°	271°	− 0·5°	394°	− 1·0°	6
—	—	—	—	—	—	—	—	394°	− 1·0°	7
—	—	—	—	—	—	—	—	—	—	8
—	10·0°	—	3·0°	—	6·0°	—	11·0°	—	11·0°	—
194°	—	137·5°	—	157°	—	271·5°	—	395°	—	—
187°	+ 1·5°	136°	− 0·5°	153°	− 1·0°	284°	+ 2·0°	—	—	9
181°	− 4·5°	137°	+ 0·5°	153°	− 1·0°	280°	− 2·0°	—	—	10
189°	+ 3·5°	136°	− 0·5°	154°	nil	284°	+ 2·0°	—	—	11
187°	+ 1·5°	137°	+ 0·5°	156°	+ 2·0°	282°	nil	—	—	12
185°	− 0·5°	136°	− 0·5°	154°	nil	282°	nil	—	—	13
185°	− 0·5°	137°	+ 0·5°	154°	nil	282°	nil	—	—	14
—	—	—	—	—	—	—	—	—	—	15
—	8·0°	—	1·0°	—	3·0°	—	4·0°	—	—	—
185·5°	—	136·5°	—	154°	—	282°	—	—	—	—

of observation from arithmetic mean of all observations.

flash-point determinations on wet oils showed very much higher variations than on those made on the dry oils. In general it is evident that these variations are greater the wetter the oil, and the higher its flash-point as measured on the dry oil. It is clear from these results that the presence of moisture does seriously affect the accuracy with which the flash-point of an oil can be determined, and as the instructions for the use of the Pensky-Martens flash-point apparatus state that the oil tested must be dry, the question naturally arises as to whether an oil can be dried before testing, and if so whether the results of test on the oil in this dried state are of any real practical value.

As a preliminary to the consideration of this question it is desirable to call attention to some general facts concerning flash-points. Flash-point determinations are made on an oil for one or both of two reasons. They may be made to ascertain the degree of safety against fire and explosion risks, in transit, storage and use, which a given oil possesses. For such purposes experience has shown that oils used for illuminating purposes are satisfactorily safe if possessing a flash-point of not less than about 73° to 100° F. In such oils the addition of moisture to the dry oil causes little or no change in the Abel flash-point, and so their case need not be further considered here. Oil-fuels which are used for steam raising may however, under certain conditions, be exposed to temperatures considerably above those caused by any atmospheric condition. This high temperature may be attained intentionally in the oil preheaters, before the oil passes to the atomisers, or it may occur accidentally when oil leaks into the spaces below the furnaces, etc. In any case the practice arrived at from experience in use, has been to fix a flash-point of not less than 150° F. for oil used for steam-raising in the Mercantile Marine, and generally of not less than 175° F. (or for certain oils of low viscosity such as distillates from shale, etc., of not less than 200° F.) for the Royal Navy. As the results given in Table III indicate, the presence of moisture in oils possessing flash-points from 150° F. to 200° F. may give rise to a considerable discrepancy between repeat tests, and the consequence of this is that disputes over the acceptance of deliveries of such oils are liable to arise.

The second reason for the determination of the flash-point of any oil or other combustible liquid is in order that a figure of merit may be obtained which will indicate the purity or impurity, or the method of manufacture, of the material. For this purpose the flash-point determination is rather analogous to gravity or viscosity-determinations made for similar reasons. In such cases the sub-

stantial freedom of the liquid from water is also usually a condition of satisfying the specification, and hence the effect of the presence of water on the flash-point is not of practical importance.

We return then to the question of deliveries of oil fuels required to possess flash-points of over 150° F., 175° F. or 200° F., as the case may be. How can disputes as to the results of discordant flash-point tests caused by the presence of water in such oils be best avoided? Attempts have been made to get over the difficulty by removing the water before testing, as is required by the directions for the use of the Pensky-Martens apparatus. This may be done by either of three methods:—

1st by treating the oil with dry calcium chloride;

2nd by heating the oil to 90° F., and filtering through English filter paper;

3rd by distilling the oil with or without the assistance of an air current, so as to remove all moisture, any light oil distilling over being separated from the water, and returned to the dry oil.

The author has tried all these methods, and has found the first to be unsatisfactory except with very fluid oils. The drying action is always slow and with thick oils very slow. The second method certainly readily removes the greater part of the water. As an example, a creosote (196 A/18) containing 6·6 per cent. of total water, of which 0·5 per cent. was in an emulsified form, was heated to 90° F., and filtered, after which only 0·35 per cent. of water was found to be present. Owing, however, to the necessary exposure of the oil to the air at a temperature of 90° F. during filtering, a serious cause of discrepancy is introduced. With thick oils, and those containing bitumen, filtration is a lengthy proceeding, and this cause of error is therefore more marked in such oils.

The third method of drying, namely by distilling at 200° F. with the aid of a current of air, necessitates a somewhat lengthy procedure, and at first sight might be supposed to give incorrectly high values for the flash-point, owing to the probability that some loss of portions of the more readily vaporisable constituents must occur during distillation. This, however, apparently is not always the case, as may be seen by reference to Table IV, where, if the pairs of the highest, lowest, and mean results of test given in the last lines of columns C and E and of columns K and M are compared, it will be seen that in three of the six pairs, the result of drying has been to lower the flash-point by from 1·5 to 7° F. In the remaining three pairs, and in the three pairs of similar results given in columns F and H, the flash-points are shown to have been raised by drying by distillation by from 1·5° F. to as much as 14° F.

TABLE No. IV.—Effect of drying on the flash point of wet oils as measured by the Pensky-Martens tester, all in Fahrenheit degrees.

Description of oil	Creosote.		Creosote.			Shale oil.			Texas oil fuel.		
Mark on sample	196 A/18.		507 A/18.			300 V/19.			679 A/19.		
Column	A	B	C	D	E	F	G	H	K	L	M
Condition of oil as tested	Wet as received.	Heated to 90° F. and filtered hot.	Wet as received.	Heated to 90° F. and filtered hot.	Dried by distillation.	Wet as received.	Heated to 90° F. and filtered hot.	Dried by distillation.	Wet as received.	Heated to 90° F. and filtered hot.	Dried by distillation.
Percentage of water present in oil as tested	6·82 %	0·4 %	0·42 %	0·2 %	Nil	under 0·05 %	under 0·05 %	Nil	1·5 %	—	Nil
	5·3 %	0·3 %	0·35 %	0·15 %	—	—	—	—	—	—	—
	8·2 %	—	0·50 %	—	—	—	—	—	—	—	—
Results of repeat flash-point determinations made by independent observers.											
Ref. No.											
1	194°	181°	174°	179°	172°	266°	268°	284°	241°	241°	235°
2	171°	190°	171°	176°	171°	270°	268°	280°	223°	234°	237°
3	190°	189°	167°	178°	169°	277°	275°	284°	241°	235°	239°
4	196°	192°	171°	174°	169°	271°	271°	282°	228°	246°	241°
5	189°	180°	174°	176°	169°	273	271°	282°	232°	235°	237°
6	185°	192°	172°	174°	169°	271°	268°	282°	248°	241°	235°
7	174°	187°	172°	178°	169°	—	—	—	—	—	—
8	172°	—	171°	—	—	—	—	—	—	—	—
Highest	196°	192°	174°	179°	172°	277°	275°	284°	248°	246°	241°
Lowest	171°	180°	167°	174°	169°	266°	268°	280°	223°	234°	234°
Mean	184°	187°	171·5°	176·5°	169·5°	271·5°	271·5°	282°	235·5°	240°	237°
Range	25°	12°	7°	5°	3°	11°	7°	4°	25°	12°	7°

In any case the results of tests given on Table IV show quite clearly that any oil which has been dried, either by filtering at 90° F. or by distillation, gives considerable more concordant repeat flash-point tests than is the case with the corresponding wet oil from which it has been obtained, whilst for the three oils containing 0·4 per cent. of water or over, the mean result of tests on the dried oil is generally, but not always, higher than the mean flash-point as measured on the corresponding wet oil. This result would appear to indicate that in general the presence of water lowers the flash-point of an oil, if it were not that the lowering of the flash-point might be equally well explained by supposing that the method of drying has itself raised the flash-point by removing some of the more volatile constituents of the oil. In order to settle this point more definitely, four oils were completely dried by distillation, and the flash-points of these dried oils were determined. Half a per cent. of water was then added to each of the oils, the mixtures of oil and water were well shaken up, and the flash-points of these mixtures were measured, whilst lastly, the mixtures of oil and water were allowed to settle for three days, and a third series of flash-point tests was made on the supernatant oils. The results of these tests are given on Table V, and show that the presence of water in the oils raises their flash-points, and therefore demonstrates that the apparent raising of the flash-points of some wet oils when they are dried must be due, not to the removal of the water, but to the accompanying removal of volatile constituents of the oils by the drying process itself.

The results of test given on Table V do not deal with all classes of oil, and as there are theoretical grounds for considering that the presence of water may possibly cause the flash-point of an oil to be lowered, it is considered desirable to give further results, in Table VI, of tests made on samples of Scotch shale oil and Trinidad fuel oil, which appear to show that in these oils a lowering of the flash-point by the addition of water to the dry oils actually occurs. As, however, the results have not been made in multiple, this apparent lowering of the flash-point by the addition of water may only be due to the accidental masking effect of the variation in flash-point determinations which, as has been shown, are very evidently caused by the presence of water, especially in high flash-point oils when using the Pensky-Martens tester.

Time has not permitted the examination with multiple repeat tests of larger numbers of different classes of oils, and before it can be decided definitely that the addition of water to a dry oil always raises its flash-point, such tests must be made.

TABLE No. V.—Change of flash points caused by the addition of water to dry oils. Results are all given in Fahrenheit degrees.

Description of oil	Light shale oil.			Mineral lighting oil.			Creosote.			Creosote.		
Mark on sample	318 V/19.			319 V/19.			2872 A/18.			863 A/19.		
Tester used	Pensky-Martens tester.			Abel tester.			Pensky-Martens tester.			Pensky-Martens tester.		
Column	A	B	C	D	E	F	G	H	K	L	M	N
Condition of oil as tested	Dried by distillation.	0·5 % of water added to dry oil A and well shaken.	B after settling out for 3 days.	Dried by distillation.	0·5 % of water added to dry oil D and well shaken.	E after settling out for 3 days.	Dried by distillation.	0·5 % of water added to dry oil G and well shaken.	H. after settling out for 3 days.	Dried by distillation.	0·5 % of water added to dry oil L and well shaken.	M after settling out for 3 days.
Percentage of water present in oil as tested	Nil	0·5 %	0·5 %	Nil	0·5 %	0·5 %	Nil	0·5 %	0·5 %	Nil	0·5 %	0·5 %
Result of repeat flash-point determinations made by independent observers.												
Ref. No. 1	153°	162°	160°	136°	137°	136°	198°	208°	189°	187°	203°	192°
2	153°	154°	156°	137°	138°	133°	201°	216°	194°	181°	203°	194°
3	154°	156°	154°	136°	137°	136°	201°	196°	199°	189°	201°	199°
4	156°	156°	158°	134°	138°	138°	198°	199°	198°	187°	201°	194°
5	154°	156°	154°	136°	138°	137°	194°	199°	199°	185°	192°	196°
6	154°	163°	156°	137°	138°	138°	196°	194°	194°	185°	198°	189°
Highest	156°	163°	160°	137°	138°	139°	201°	216°	199°	189°	203°	199°
Lowest	153°	154°	154°	134°	137°	136°	194°	194°	189°	181°	192°	189°
Mean	154°	157·8°	156·3°	136°	137·6°	137·3°	198°	202°	195·5°	185·7°	199·7°	194°
Range of variation	3°	9°	6°	3°	1°	3°	7°	22°	10°	8°	11°	10°

TABLE No. VI.

Examples of dry oils the flash points of which appear to have been lowered by the addition of water.

Description of oil	Scotch shale oil.			Trinidad fuel oil No. 3.		
Mark on sample	550 T/14.			268 A/14.		
Tester used	Pensky-Martens.			Pensky-Martens.		
Column	A	B	C	D	E	F
Condition of oil as tested	Dried by distillation.	0·5 % of water added to dry oil A and well shaken.	B after settling out for 3 days.	Dried by distillation.	0·5 % of water added to dry oil D and well shaken.	E after settling out for 3 days.
Percentage of water present in oil as tested	nil	0·5 %	0·5 %	nil	0·5 %	0·5 %
Observed flash point	239°	231°	234°	183°	174°	176°
Effect of added water on flash point	—	Lowered 8° F.	Lowered 5° F.	—	Lowered 11° F.	Lowered 7° F.

Attention is called to the tests on dry and wet oils made by the Abel tester on mineral lighting oil (319 V/19) recorded in columns D, E and F of Table V. These show that the presence of moisture in low flash-point oils produces little or no effect on measurements made by this tester, whilst even dry oils tested on the Pensky-Martens tester, when their flash-points are high, give results variable by as much as about 8° F.

The author possesses a good deal of evidence which shows that the amount of water which can dissolve, either in creosote or in petroleum and shale oils of all descriptions, is very small, probably less than 0·03 per cent., and therefore any water present in excess of this amount must be in the form of more or less minute globules. These globules are held in suspension more readily the more closely the density of the oil approaches that of water, as is the case in creosotes, whilst clearly the high viscosity of thick fuel-oils and tars must also assist to prevent the liquid globules from separating out. It is believed that an inspection of the results given on Tables III, IV and V will lead to the view that it is probably the presence of these liquid inclusions of water in oils (which may be presumed to be irregularly distributed) which cause the large variations in repeat flash-point tests made on wet oils.

Having examined the effect of drying oils by various methods before measuring their flash-points on the Pensky-Martens tester, the question arises as to whether there is really any practical advantage to be obtained by carrying out such preliminary drying treatments.

Even if the drying of an oil could be carried out by a process which did not alter the oil itself in any way, the author considers that there are at least three good reasons for not drying an oil before measuring its flash-point.

In the first place, the information which it is required to obtain concerning an oil by determining its flash-point is as to whether it is more or less safe or dangerous as regards the origination of fires or explosions, and this information is required concerning the wet oil in the condition in which it is to be handled, and not concerning some product of laboratory manipulation derived from the oil, even if it is possible to demonstrate that this dried oil really possesses the flash-point of the oil before it was mixed with water. The dehydration of an oil cannot occur accidentally in the course of handling it, and as a matter of fact can only be carried out intentionally, even on small samples of a gallon or so, with a good deal of difficulty, whilst to dehydrate a quantity of a ton or more would require very special plant. There is no doubt therefore that a wet oil will remain wet throughout its commercial existence, and it is the flash-point of this wet oil which is important.

Secondly, the procedure for drying an oil before measuring its flash-point is certainly an undesirable complication in laboratory work, whilst it introduces a serious extra source of experimental error, for the dehydration may and generally does cause the flash-point to be raised, owing to the escape of the more volatile constituents of the oil, and if great care is not used in carrying out the drying treatment, such errors may become very large.

Lastly, the real difficulty caused by discrepancies observed in repeat flash-point tests made on wet oils is the inconvenience of disputes arising between the buyer and seller. In the author's view, this difficulty can best be met by testing the oil in the wet state. Then, if the amount of water present is not greater than about 0·5 per cent. and if the flash-point is not greater than 200° F., the maximum variation between properly-conducted repeat flash-point tests will be but little greater than on a dry oil of a similar character, which, using the Pensky-Martens tester as may be seen from the results given on Tables III, IV and V, is not greater than 8° F.

For fuel oils delivered under Admiralty specifications, the above conditions would certainly hold, for such oils are required not to

contain more than 0·5 per cent. of water, and the minimum flash-point is fixed at either 175° F. or 200° F. according as the oil is of high or low viscosity. For tests made on such oils in the wet state, there may therefore be a legitimate difference between the buyer and the seller's results of 10° F. In the author's opinion, dispute over such discrepancies can be readily avoided by making the specified flash-point ten degrees higher, say, 185° F. and 210° F., and then allowing a tolerance of 10° F. in accepting deliveries. By this procedure the degree of safety of the oil as regards the possibility of fire or explosion will be fully maintained, whilst the inconvenience of dispute will be eliminated. To give an example: If a delivery of an oil is specified to have a flash-point of 185° F., and is found on test by the buyer actually to possess a flash-point of 175° F. it would be accepted without remark, but if it is found to possess a flash-point of say 171° F., it would be rejected not because it has a flash-point lower than 175° F., but because the flash-point is lower than the specified 185° F. Under such conditions it is considered that a rejection could be easily maintained without dispute.

The frothing of fuel oils containing water, when under test for flash-point, is clearly of no importance, for frothing cannot take place until the oil is heated above 200° F., and if no flash is observed at this temperature the test need not be carried further. It is believed that no existing specification for fuel oil requires a higher minimum flash-point than 200° F.

In conclusion, the author desires to express his indebtedness to Mr. Wycliffe Killner and Mr. F. H. Newington for their assistance in supervising the performance of much of the quantitative experimental work recorded in this paper.

DISCUSSION.

The Chairman said the very best thanks of the members were due to the author for his valuable paper. The time available for the discussion was all too limited, and therefore he would only say that as one result of Mr. Arnold Philip's paper the subject of standardisation might be followed up much more closely, and if the Institution could give any help in that direction he believed it would do so. There was of course an international side to the subject, but he imagined that the first endeavour would be to see what amount of system could be brought into the methods of the Institution. Just before the meeting he had had a talk with Sir John Cadman, who had said that the reading of the proof of the

paper had been of considerable value to him in some work on which his department was engaged, and that he hoped he might be present that evening, from another meeting, in time to take part in the discussion.

Sir Thomas Holland, in response to the Chairman's request, said that he had no intention of contributing to the technical side of the discussion, but was glad of the opportunity of expressing his pleasure in meeting so many old friends again after his long absence. Many of the members had been engaged in war work, and he, like them, had been dragged away from the subject, not only of the paper, but of petroleum technology generally, as he had been kept in India in an endeavour to complete a task which the Chairman himself had come out to set, and had then run away!

The subject of the paper fell into two parts, first that part appealing to those who were interested in the proposed Standardisation Committee which was suggested by Mr. Duckham, a valuable suggestion which had been lying fallow for something like two years, and the other part the purely technical points raised which he had no doubt the chemical technologists present would take up and analyse as a definite contribution to the question of standardisation.

The question of standardisation was one in which he was very much interested, as he supposed every member was and indeed ought to be. There were several preliminary difficulties which had to be got over. First of all there was the British character, which objected to being standardised; that was one of its good features, and also one of its defects. Without going to an extreme, it was possible at once to obtain a certain amount of practical value by establishing standards consistent with vested interests already established. It was also necessary to consider that by postponing attempts to standardise methods for something like perfection, they would only allow the growth of further vested interests and therefore of difficulties which would subsequently block the way.

In the first place, it was not only members of this Institution who would be affected by the proposal, but it would be necessary to work in concert with the chemical Societies and Institutions that had already done a considerable amount of work in that direction, and who were interested in one phase especially of chemical standardisation. There was also the question the President had alluded to of International co-operation, which was one of great importance. The Institution need not postpone action until they had got all other countries into full co-operation, but it might be worth while to consider the desirability of bringing in the more important petroleum countries, especially America,

which used a form of our language and in other ways had helped so recently to standardise international morality. It was desirable, therefore, that instead of acting independently in this country, they should obtain the enlightened experience of America, and move along lines of mutual advantage.

It was not possible to set up standards without much experimental work, and work could not be done without being paid for. The British Engineering Standards Association, as was known, spent considerable sums every year in establishing engineering standards, and a portion of the work in connection with petroleum products would actually touch on the Engineering Standards programme, especially in connection with lubricating oils. It was therefore necessary to consider the question of who was to meet the bill, whether it should be met by private effort or whether it would be chargeable fairly on Government funds.

The first thing to do, although he was rather loath to make the suggestion, was to form a preliminary Committee. He hesitated to propose the formation of another Committee, because it seemed to him that committee-making was the one industry that flourished at home during the war, but it was possible that by forming a preliminary Committee that would gather together what had been done already, and set out some lines for further action, it would be possible to move a little more safely, instead of moving full speed ahead, and accepting the proposal, which had been made for the establishment straight away of a standardisation Committee.

He did not think there was a member present who could discuss the question of standardisation without considering a matter which must be uppermost in the minds of all of them at this first meeting of the session. There was nobody in the petroleum world who, by his own personality and his own research work, established recognised standards so much as Sir Boverton Redwood himself. In many ways his standards were used to-day in different parts of the world, and he was perfectly sure that everybody present in thinking of standards must realise what an enormous amount of work Sir Boverton had done, and how by his personality and through his writings he had established standards which were being followed, either partially or wholly, in various countries. Even the Abel flash-point apparatus was really the product of Sir Boverton Redwood's researches, and from his work followed the conclusions on which were based some of the petroleum regulations in India.

The establishment of a Standardisation Committee ultimately would be exactly in accordance with what Sir Boverton Redwood himself would have wished, and the best thing the members could

do to show their respect for his memory and for the great work he did, was to support the Institution which was one of the latest products of his public spirit.

Dr. Dunstan said the paper indicated the vast amount of work and the careful attention to detail necessary in standardising procedure. The author had made a most valuable and authoritative contribution to the sections of analysis dealt with, and his views must carry very considerable weight. The paper was so full of interesting possibilities for discussion that it became extremely difficult to select even a few for adequate treatment. The first point to which he would refer to was that in any question of standardisation it was fundamentally important to define one's quantities. The author had referred to the ambiguity of recording the temperatures in two scales, but he would point out a further want of uniformity. The specific gravity of an oil was talked about in a very casual and careless fashion; whether it meant the absolute density, or the relative density against water at 0° or 4° C. or the temperature of the determination, was never made clear; therefore he suggested that in all cases regard should be paid to the more scientific standard of absolute density.

It was well known that many thermometers were used much too soon after their manufacture, and it became a matter of extreme importance to re-calibrate or re-standardise the thermometers on which accurate work depended. It was usually found that most thermometers were standardised for total immersion, which in actual practice was never the case. These were points that any Standardisation Committee should bear in mind at the outset.

Dealing with some of the detailed work set forth in the paper, he might mention first the question of the topping of crude oils, in the ingenious apparatus which had been shown on the screen. He did not quite know why the author cut for kerosene at so low a temperature as 260° C. measured in the oil, and on what grounds he considered a current of air as better than the customary procedure of a current of steam. His experience was that steam (introduced, of course, after removal of benzine) gave a far better distillate and with less liability towards cracking. He had been very interested to hear the author's views on the actual cutting-point, because it occurred to him that the cutting-point was to a certain extent predetermined by the qualities the residue had to possess.

He should be interested to hear the author's views on the question of carrying out the primary topping process with the aid of a dephlegmating column. He took it that in the second distillation referred to, from the tops, the author did work with a column, and

it would be a matter of interest to know whether he considered that Works practice yielded higher percentages of products than those obtained from a careful laboratory examination.

A further consideration of some importance occurred in connection with the quantities used. Naturally in a preliminary investigation of crude oil, one would like to work on as big a scale as possible. Occasionally one could not do that owing to a limited supply. An illustration occurs in connection with the author's analysis of the Hardstoft oil. He had come across a quantity of that oil some time ago, to the extent of about an ounce, on which were carried out the ultimate analysis, sulphur flash-point, calorific value, and distillation tests and the figures were as follows as compared with the author's figures on Table I. He would quote alongside the figures those of Mr. Hackford, who had published his results this year. The author's total benzine from 1214 cc. was 4·8%. From 30 cc. of the crude the speaker obtained 5% of benzine, and Mr. Hackford 4·5 % of aviation-benzine. The author's kerosene oil was 37·5, the speaker's 32, Mr. Hackford's 33. With regard to the lubricating oils the author's figure was 26·75, Mr. Hackford's 29, and the speaker's 30. It was therefore evident that it was possible to get a fairly comparable set of results even when working on a minute scale.

The estimation of wax was touched upon in the paper, and it would be interesting to hear what the suggested method was that the author referred to. He should like to ask him whether he had tried the Bureau of Mines' method, which consisted in distillation, extraction of the distillate with oleum, adding alcohol, refrigerating and filtering; it was referred to as a very accurate method if certain corrections are made. His own preference lay in the direction of treating quite a large quantity of oil, 5 or 10 gallons, and working up the wax practically on commercial lines.

On the subject of calorific values he was entirely in agreement with the necessity for fixing the large calorific values, but always preferred to do a complete combustion of oil as well. Firstly, because the data on the ultimate analysis of crude oils was very scanty, and secondly because the quantity of oxygen, nitrogen and sulphur in the crude oils gave a kind of rough-and-ready check on the true calorific value.

Finally he would like to suggest that if ever the Standardisation Committee got to work it should include in its purview those more recent oils, the low-temperature tar-oils, cannel oils and lignite oils, which undoubtedly had an exceedingly important future for us.

Mr. Mitchell said he should like to take the opportunity of endorsing a good deal of what Dr. Dunstan had said with regard to the calorific value, and the importance that should be attached to the ultimate analysis taken in conjunction with that. It was as far back as February 1911 that Mr. W. Inchley published a paper in the "Petroleum Review" in which he gave some very interesting formulæ, showing how it was possible in many instances to calculate, merely from the carbon and hydrogen contents of the oil, its approximate calorific value. Mr. Inchley took nothing into consideration with regard to the amount of oxygen or inerts like nitrogen that might be present in the oil, and it was rather remarkable how nearly the calculated results approximate to those obtained in the calorimeter. He himself had taken the trouble to calculate out for some of the substances that were described in the paper, notably di-ethylether, what the calculated value would be according to Inchley's formula, and there was a discrepancy of some 7%; but in that substance there was some 22% of oxygen. One statement with regard to calorific value he did not quite follow. The author said: "The water formed from the combustion of the combined hydrogen in a ton of fuel varies from a minimum of ·3285 tons for a French non-caking coal up to 1·494 tons for pentane. Therefore in burning a ton of fuel it must evaporate from ·3285 to 1·494 tons of water derived from the combined hydrogen which the fuel contains." That was rather a terminological inexactitude. The hydrogen combined in the fuel was certainly not there as water; it was combined with carbon, and at the temperature of combustion liquid water would not be formed, so that he did not see how that statement quite tallied with facts. With regard to testing for free fatty acidity the difficulty he had always experienced in testing for free fatty acid, especially in the cold, was the influence of CO_2 on the indicator. It was rather a protracted operation, and the CO_2 in the air rapidly influenced the indicator. A time of 9 hours was mentioned as being the time sometimes taken by the operation, and in that time it would be very difficult to keep out atmospheric CO_2 to a sufficient extent to ensure that the results would not be vitiated. By working rapidly at a fairly elevated temperature, and keeping out the atmospheric CO_2, he had been generally able without much difficulty to get the free acidity in the course of about fifteen minutes. With regard to the flash-point of an oil being occasionally not very much altered by distillation, he thought very likely there was a kind of balance between two tendencies. On the one hand there was probably a loss of some of the very lightest of the oil, and that was balanced by a certain amount of cracking

of the rest, the cracking producing light constituents which helped to keep down the flash-point. The author had cast a reflection on human nature by remarking that probably in petroleum refineries oil that had been in contact with sulphuric acid might be used as fuel. He should not like it to be taken as any adverse reflection on his human nature, but he had certainly seen that done as quite a daily operation. Tar from the kerosene washers had been run down into separating tanks, where it had been well washed with water, especially the soda-water from the soda washing-tanks, and then in some instances it had been used as fuel, and in some cases it had been good enough to return to the stills for re-distillation and had given quite a good enough kerosene oil for admixture with the cheaper grades.

Prof. J. S. S. Brame said that the very suggestive paper they had heard was hardly within the scope of discussion in detail, nor did he think it desirable to criticise it from that point of view. It was a subject which should be thrashed out by the Committee which he hoped to see appointed. He supported the idea of the Committee for investigation, but as Sir Thomas Holland had pointed out, the £ s. d. question had to be taken into consideration. If the Institution succeeded in obtaining funds the Council, would do its best to see that the money was properly expended. It had been pointed out both by the Chairman and by Sir Thomas Holland that it would be necessary to co-operate with other Societies and other countries. This country was not producing petroleum in very large quantities at the present time and therefore could not rank as a first-class oil producer, but there were many chemists and technologists in this country specifically interested in Companies producing largely in other parts of the world, and therefore the Institution should take its share in the question of standardisation. It was most important to remember that the United States had already gone a considerable distance in the matter, and some very excellent work had been carried out in the laboratories of the United States Bureau of Mines, for example, on the distillation tests of petroleum by Rittman and Dean, and could be found in the *Bulletin U.S. Bureau Mines*, no. 125. 1916. He did not propose to deal with the results obtained by those competent chemists, but it was of no use for any chemist to adopt a form of still which worked very well from his point of view, without taking into consideration such facts and conclusions as were arrived at in this extensive investigation.

The International Congress on Applied Chemistry had dealt with the two very important topics of standardisation of flash-point

methods and also with viscosities. It was necessary to move in co-operation with other people.

Mr. E. L. Lomax congratulated the Institution on receiving such an interesting paper, and especially as it gave the views of another side of the petroleum section of the Institution, the side of the large consumer as against the large producer. No real progress could be made in such an Institution unless both sides were considered. Although he did not altogether agree with the methods of the author, there were a good many things to be said for them. In listening to a paper of the kind one saw what one was up against in supplying oil to large consumers using such definite tests. On many occasions there had been very serious quarrels between the chemists of the producers and the chemists of the buyers on account of differences in tests owing to the fact that the tests were not standardised. It was only by discussing those tests in the way that they were discussed in the paper that methods could be arrived at which would give concordant results in the hands of different investigators.

With regard to the question of preparing representative samples of oil, he should like to ask the author if he had ever used what was known as a "thief" for that purpose, i.e., a tube with a valve at the bottom and by which what was practically a core of the oil could be taken out of the sample. He liked the author's method, but it was rather lengthy, and when one had to give results the next day, one could not spare the time for so long a method.

Again, with regard to distillation tests with crude oil, he was afraid he did not agree that the method should be reduced to such a point that the personal skill of the investigator was of secondary account, particularly when new crude oil was being valued. The personal skill and experience of the investigator in that case was often of much more value than the actual test. Undoubtedly the new test devised by the author reduced the possibility of variation to a minimum, but he questioned whether it would be useful to the producer in evaluating a crude oil for himself.

The method of distillation by the use of air was not one practised in refineries as a rule, and very different results were obtained in those distillations from what were obtained when using steam. One was very liable to crack an oil when using air, a thing that would not occur when using steam. Some crude oils were quite unstable at such a high temperature as 260° C., and unless special precautions were taken in the distillation, considerable cracking would take place. Then in the second distillation, the cooling down and heating up was another source of cracking, and one

could not possibly help it in a case like that. He thought the fact of cracking was particularly emphasized by the samples of wax which the author mentioned. In America it was quite a common practice to crack the fractions of kerosene to a certain extent in order to aid the better separation of wax; when that took place the wax separated out in a very crystalline form. The wax produced in the author's distillation being in that definite crystalline form showed that a certain amount of cracking must have taken place, and it was less marked or even absent in the tests carried out by other operators.

With regard to the estimation of calorific value of motor spirit, another factor was introduced in the method suggested, and that was the question of naphthalene. This was a pure substance and its calorific value was known, but certainly an error was liable to be introduced, and he had wondered if the author had ever tried the use of a Lunge pipette for putting the petrol into the combustion capsule, using the pipette in the manner described by Lunge for fuming sulphuric acid. An accurate estimation of the spirit introduced into the bomb can be made, and by closing the bomb quickly loss is avoided and no other source of error introduced.

With reference to the effect of moisture on the flash-point of a mineral oil, the author did not seem to have come across two papers, in which experiments were carried out as to the effect of added water on the flash-point of an oil, one by Matuschek (*Oesterr. Chem. Zeitschr.*, iv, 209. 1901); the other by Kharitchkov (*Viesln. Jirov. Vesch.*, viii, 60. 1907). The first observer obtained a maximum flash-point of 32° C. from an oil of original flash-point 21° C. with the addition of 1·4 % of water. With further addition of water the results were very erratic. It was evident from the results obtained by the author and different workers that one of the things that could be well gone into was the question of the effect of water on the flash-point of an oil.

Dr. Ormandy said that one of the outstanding features of the interesting paper was the fact that on reading it through one found a very large number of experiments had been made, and yet, throughout, the author was always most diffident in expressing anything like a positive opinion. In the position which the author held as the Chief Chemist to the Admiralty, he held great power, and one would expect him to speak with a considerable amount of certainty, instead of which he exhibited a degree of scientific honesty which was really unusual in the best scientific papers; and one began to wonder whether it would not be possible to lend Mr. Philip to the Telephone Department or to the Treasury!

There was one feature of the paper to which he would like to direct attention. It would be noticed that the author referred in parallel columns in several cases to experimental work done first of all on petroleum oils, secondly on shale-oils and thirdly on tar-oils, and elsewhere in the paper he referred to the imminent necessity for determinations of moisture and so on in fuels which might contain alcohol, which would of course contain water. That appeared to show that the functions of the Institution were not confined only to petroleum in the ordinary sense of the word. He thought it would be a very good thing if the members of the Institution would remember that, and, in endeavouring to help to increase the scope of action of the Institution by bringing in new members, would convey to people, outside those to whom the Journal transactions usually went, that the Institution of Petroleum Technologists was not specifically and only intended to deal with oil obtained from the ground, known as crude petroleum, but was equally interested in the products of the distillation of minerals, such as shales and coal. He thought that was not generally realised by the public. Looking through the list of members, it would be found that the tar-distillers, the coke-oven people and the blast-furnace people were very poorly represented indeed. It was desirable that the activities of the Institution should be increased, and that necessitated funds, as had been said. When new members were obtained not only did it mean more money, but it meant that at the same time the scope and power of the Association were increased, and that was one of the points which the paper had brought home to him.

With regard to the details of the paper, there were many things one could speak of, but one point he wished to allude to was the question of the vapour-pressure method of determining what the author spoke of as the "boiling-point." Personally, he did not attach much importance to that figure, because, taking for instance a mixture of paraffin oil, which would have at any ordinary temperature no considerable vapour temperature—speaking of low temperatures up to 30 or 40° C.—if that paraffin oil were mixed with a very small amount of low-boiling petroleum ether or ordinary ether, one would obtain quite a high reading in the vapour-tension apparatus, that was to say, there would be a high pressure at a very low temperature, and yet the boiling-point curve of the liquid might show two or three drops at a low temperature, and nothing more coming over until the ordinary temperature at which paraffin was distilled was reached. It seemed to him that in the future there were going to be very much greater difficulties in that connection,

because undoubtedly, starting slowly, probably in our tropical dependencies, but gradually coming this way, the utilisation of mixed fuels, alcohol-benzol and alcohol-ether, would come forward. In Australia they were already contemplating the putting down of plant to make 2,000,000 gallons of alcohol fuel per annum, and in India a large plant was being installed, and the question of the peculiar behaviour of mixed volatile fuels, such as benzol-petrol and alcohol-benzyl-ether, would have to be considered and methods of analysis worked out. It would be necessary to face quite a new series of difficulties, because, as some of the members probably were aware, the question of alcohol in its potable form and its ability to mix with water was sometimes taken advantage of by the "trade," and in the days to come, when whisky was going to be put in the tank of the motor car, there was just the possibility that a knowledge that alcohol and water did mix might cause application to be made to the pump, and the petrol dealer or motor fuel dealer of the future might have a reputation which would be on a par with that of the milkman! At any rate, it did point out to necessity for the Institution, conjointly with others, taking some action in the direction of helping to standardise methods which were required now, and which would be more required in the future. The thanks of the Institution were certainly due to the author for having brought a number of points before the meeting to illustrate that in a very vivid manner.

The Chairman said the author did not propose to deal in detail with all the suggestions made, but had promised to send in further remarks in writing. He announced that the Council was holding a special meeting on Tuesday, November 4th, to consider in what manner the Institution might best extend its usefulness, and they would welcome any suggestions from any member of the Institution which would help them in that matter.

Mr. Arnold Philip said that, as Sir Frederick Black had stated, it was not possible at such a late hour to make a detailed reply to the lengthy criticisms of the paper, and he felt he would be able to reply more satisfactorily when he had had the advantage of seeing the speakers' remarks in writing. But there were two points he would like to mention at once. One was with regard to Prof. Brame's plea that the results of American investigators should be fully considered. The claim of the paper was really that everybody's results should be considered, irrespective of nationality. He did not arrogate for a moment any special merit to the described processes except that they were the best he knew of. He thought the final decision as to standard processes must be arrived at

by the advice of some Committee on standardisation, and he would be prepared to sacrifice every process he had devised if the Committee were satisfied that another was preferable. He agreed with Dr. Ormandy that the U-tube method of comparing boiling-points of motor spirit gave results of no very great importance, but they were figures of merit for spirit, and, although a sample might contain only a very small proportion of low flash-point or high vapour-pressure material, this did affect its pressure in storage and its explosiveness. This figure of merit could be readily and quickly recorded, and was from that point of view preferable to the distillation-point, which was very wide in variation, giving enormously discrepant results in repeat tests, and of little or no value if consistent.

Mr. E. A. Evans subsequently wrote as follows:—

It has been my desire, no doubt in common with many others, to learn something about the laboratory methods used by the Admiralty Chemist, hence I heartily appreciate these very explicit details which he has so kindly given to us, I only regret that he has been unable to tell us more.

The accurate estimation of water in oils is one of great difficulty. The simplicity and the claimed accuracy of Mr. Philip's method must commend itself to all. I venture to think, however, that the method when used to determine very small quantities of water may be open to criticism. I am thinking especially of "dry" transformer oils, where traces of water very materially affect the dielectric strength.

Transformer oils will oxidise and produce water when a current of air is passed through them, therefore, it is possible that small amounts of water would be generated during the test and so vitiate the results. This difficulty could be overcome by using an inert gas in place of air.

In view of the probability of reclaimed oils coming on the market a word of caution might conveniently be made here to remind chemists that the water-soluble acid is not necessarily an inorganic acid. In the case of used oils one frequently finds water-soluble organic acids. The free acidity to alcohol is not always quite straightforward. After titrating in the presence of a few drops of phenolphthalein I have found that on the addition of a few more drops of the indicator a further quantity of sodium hydroxide is required. Perhaps Mr. Philip may have encountered such cases. It appears to me that if there be insufficient separation of the oil from the alcohol to mask the colour of the indicator it would be more satisfactory to extract the oil with alcohol and then titrate

the alcoholic extract. Many mineral oils will absorb a certain amount of sodium hydroxide when heated with it, so obviously to allow the oil and sodium hydroxide to remain together hot for a prolonged period must raise doubts as to the accuracy of this procedure.

Unfortunately the determination of flash-points is often relegated to a minor assistant on the assumption that it is a simple test and requires little skill. During the war I had occasion to enquire rather more closely into flash-point determinations than I had previously done, and I was amazed at the result of my enquiries. I sent samples of the same oil to several chemists well respected in the oil world. There were 100° C. between the highest and lowest figure obtained. One laboratory reported 327° F., 676° F. and 448° F. on the same oil! Surely the presence of water in the oil would not solely account for such differences. I visited some of the laboratories, and was horrified at the condition of the flash-point apparatus and the manner in which they were used. I should judge that no definite rule exists as to the effect of water upon the flash-point of an oil, sometimes it lowers the result by what amounts to steam-distillation of the lower members, and sometimes raises it preventing ignition of the oil vapour.

Mr. Philip states that the solubility of water in creosote, petroleum and shale oils is probably less than 0·03 %. I am not quite clear whether he intends to suggest that the variety of the oil does not influence the solubility or not. Groschuff (*Zeitschr. f. Electro-chemie*, xvii, 348. 1911) has found the following solubilities:—

Temp		Benzene.		Kerosene.
20° C.	...	0·061%	...	0·006%
50	...	0·161%	...	0·024%
94	...	—	...	0·097%

and states that the solubility of water in transformer oil is about three to four times that of water in kerosene.

Mr. Philip, the author, wrote as follows in reply:—

He fully concurred with Sir Thomas Holland as to the great difficulties which surrounded the subject of the standardisation of methods of test on oil. The descriptions of methods a few of which he had attempted to give in his paper were only the tedious but necessary preliminaries in which many must share if the proposed standardisation of methods of test was to be a success. The greatest difficulties would arise, as Sir Thomas had indicated, when a sufficient accumulation of detailed descriptions of methods of

test had been effected to make it possible for some Committee of recognised authority to discuss them, with a view to defining some standard procedure which would be accepted by all interested in oil technology, not only in England, but throughout the world. The British objections to standardisation would unfortunately probably add considerably to the labours of any Committee which might be formed to deal with this question.

Dr. Dunstan had pointed out that the specific gravity of an oil was frequently stated in reports without also recording the temperature at which it was measured. This was of course unsatisfactory, but if the temperature was stated the specific gravity always had a perfectly definite meaning, and could be transformed into the density at the same temperature by multiplying by the density of water at that temperature. The measurement of the specific gravity of liquids at stated temperatures for technical purposes, as was now customary, would never be replaced by the use of density measurements. The reasons were too lengthy to permit of discussion at the present moment.

The reason that he topped oils by his method up to a temperature of 260° C. was that experiment had shown him that at this temperature, and using this method, he got all the kerosene over. He was quite prepared to vary this temperature, if good reasons could be shown, but he felt that there was some justification for it in the results given on Table I (pp. 24-26), where it would be noticed that all the other perfectly independent observers who had recorded the temperatures had obtained very similar results to his own.

In this particular oil, as far as the volume of kerosene obtained was concerned, his result was absolutely in accordance with that of observer A and intermediate between those recorded by observers D and E 1. He considered that it would perhaps be desirable to obtain some more definite and authoritative ruling, as to the best point at which to interrupt the distillation for kerosene, but he felt that his attempt when compared with those of other well-known experimenters who had independently tested the same crude oil was not very wide of the mark.

At Portsmouth they did not use a dephlegmator either when fractionating petrols, or when separating petrol from kerosene in the tops from crude oils. The connection between the laboratory tests and the results obtained by the works stills, he had no opportunity of knowing, and evidently this knowledge must affect the decision as to whether or not a dephlegmator should be used, and if so, what its pattern should be.

The agreement between the results for the distillation of Hardstoft

oil as obtained by Dr. Dunstan, Mr. Hackford and himself was interesting, and the fact that his own results for kerosene were higher than those of either of the other two observers was a further indication that the limit of distillation of 260° C. which he had employed was at least quite high enough. The method of paraffin-wax determination which he had used was that which he was now investigating with his colleague Mr. T. Stenhouse, and he would prefer not to describe it in an incomplete form.

The U.S. Bureau of Mines method for paraffin-wax determination appeared to be a very promising method, and he proposed to compare his results with it. Of course the tests on large volumes as used by Dr. Dunstan had the great advantage of being more likely to represent actual working conditions.

Personally he did not agree with Dr. Dunstan's advocacy of the desirability of making direct routine determinations of the hydrogen, carbon and nitrogen present in oil fuels, on account of this being usually unnecessary. If the high calorific value and the "water value," and of course the sulphur, were determined it appeared to him to be all that was usually required. The accuracy of properly-conducted calorimetric determinations was generally higher than that of combustions, but if combustions were to be made, they could best be carried out as he had stated in his paper, and as probably Dr. Dunstan would agree, by means of a calorimetric bomb. He considered that the procedure of checking the bomb-determinations by the results of ultimate analysis might be described as an attempt to paint the lily.

He fully agreed with Dr. Dunstan's views as to the advisability of including the consideration of methods of test of low-temperature tar-oils, and also cannel- and lignite-oils in the references to the proposed Standardisation Committee. He wished to thank Dr. Dunstan for his words of appreciation.

Mr. Mitchell had underlined Dr. Dunstan's views as to the desirability of checking bomb-determinations of calorific values of oils by calculations from the amounts of hydrogen and carbon which they contained as determined by combustion: whether in the bomb itself or in ordinary combustion-tubes was not stated.

This view appeared to the writer to advocate a deplorably retrograde movement to those dark times when the Thompson and Thomson calorimeters were generally used, and when the bomb had not become an essential piece of apparatus in the fuel laboratory. In the example which Mr. Mitchell had cited of the calculation of the calorific value of ordinary ether from its chemical formula, he stated that it differed by 7 % from the value obtained

with a bomb. That was a difference which was about twenty-five times as large as the experimental error usually considered as possible in bomb-tests, and in the writer's opinion illustrated, perhaps in an exaggerated manner, the objection to such calculation methods of checking.

It had been stated in the paper that the water from the combustion of the combined hydrogen in a ton of pentane amounted to 1.494 tons, and that therefore in burning a ton of this fuel 1.494 tons of water derived from the combined hydrogen had to be evaporated. Mr. Mitchell considered this statement to be a terminological inexactitude, but to the writer it appeared to be a cold and calculated truth. He felt that the word evaporation might well be used to indicate the transference of water by heat agency in the form of vapour from one point in space to another, even although the water did not originally exist in the liquid form, but in any case he did not know any alternative to the expression which he had used, which could so clearly and shortly state the actual facts of the case. If Mr. Mitchell could suggest some more satisfactory expression he would be glad to use it.

With regard to the determination of the free organic acidity of an oil he had stated that his test might occupy as long as from six to seven hours and not nine, as Mr. Mitchell stated, but this was only for very thick and dark oils of high acidity. For oils of low acidity it did not occupy more than 15 minutes. As far as Mr. Mitchell's short statement described it, his procedure did not differ from the author's. In both methods the oil was titrated hot in the presence of alcohol, but he hoped that if Mr. Mitchell could suggest some method of accelerating the determination he would do so. Although the process the writer had used occupied an undesirably long time for certain oils, practically no error was introduced into it from the interference of atmospheric CO_2. This gas was only present to the extent of 0·03 % by volume in the atmosphere, and a short calculation would show that even if the air-space in the flask, which was about 100 cc., was completely filled with fresh air 30 times during the test, and if all the CO_2 in that air was absorbed in the oil and alcohol, it would only cause the percentage of free acid calculated as oleic to be raised by 0·02 %. As a matter of fact something like this error occurred, as could be easily shown by means of a blank test made with a volume of neutral alcohol equal to that of the oil and alcohol used in the actual test. When this was heated in the same-sized flask side by side with the actual test, it was found that the alkali required to keep it coloured pink during the seven hours was only equivalent to 0·015 % of oleic

acid, calculated on the 50 grammes of oil taken, and this amount, on an oil of high acidity which took seven hours to titrate, and contained over 1·5 % of free acidity as oleic acid, was negligible. In the remark which he had made in his paper as to the possibility of oil which had come into contact with sulphuric acid being mixed with fuel oil, he meant of course oil which after its contact with acid had not been completely neutralised with alkali. On account of this possibility and for other reasons he considered that a strict watch should be kept on the acidity of oil fuels to water.

Mr. Mitchell also referred to the possibility of cracking oils when they were heated in the process of drying them for flash-point determinations. Perhaps he had not made it clear that in such drying operations it was unnecessary to heat the oil above about 200° F., if a current of air was used as in his method. But if Mr. Mitchell suggested that at such a temperature cracking could occur he should like to know upon what evidence this view rested. He had noticed that the statement that an oil had been cracked at comparatively low temperatures had not infrequently been made, without any proof being advanced that such cracking action had really taken place. To prove that an oil had been cracked was frequently not at all a simple thing to do, more especially if the cracking action were small in amount.

He had now had the advantage of reading Prof. Brame's criticisms in writing, and these, although modified in form, still appeared to give the impression that Prof. Brame feared that the writer and other members of the Institution were either unacquainted with the contributions of Rittman and Dean and other American investigators, to the literature on the technology of mineral oil testing, or that being familiar with their work they were desirous of treating it with neglect. Speaking for himself, and he believed for all other members of the Institution, he could assure Prof. Brame that these fears were groundless. He was particularly desirous of making this statement for himself, for by the courtesy of the Department of the Interior of the U.S.A. he had been regularly supplied with the Bulletins and Technical Papers published by the Department, and this formed a privilege which he valued very highly.

Mr. Lomax's references to two papers of flash-point determinations made on wet oils were very welcome. He had not previously seen these, because although abstracts of them were published in the Journal of the Society of Chemical Industry, they were not indexed either under "Flash" or "Water," but both appeared under the somewhat misleading headings of "Petroleum Influence

of Water on Flash-Point," and had thus been overlooked. He would take an early opportunity of reading the originals. Both these papers, however, as far as it was possible to judge from the abstracts, had one great fault—they did not appear to be based on the results of multiple tests, and only multiple tests were of any real value in such investigations. The abstracts occured in *J.S.C.I.*, xxi, 909 (Matuschek, 1902), and xxvi, 958 (Kharitchkov, 1907).

The use of a " thief " in some form would appear to the writer as probably the best method of sampling oil in shore and ships' tanks, but although he had heard of this method he had not had practical experience in its use. Possibly the same procedure might be useful for sampling bulk laboratory samples, but for reasons which it was difficult to discuss except at an inconvenient length, he believed that this arrangement was not satisfactory for laboratory work. The correct determination of the amount of water in oils under test was naturally of much importance, and however long it took, one was forced to spare the time to arrive at an accurate estimation of this objectionable constituent of fuel-oil. If one attempted to save time at the expense of accuracy, the whole procedure of test might become useless, and in reality much more time and incidentally labour would be wasted.

With regard to the feasibility of arriving at a final decision as to the adoption of standard methods of fractional distillation for all kinds of oil products, he felt that it would not be possible to define procedures which would be generally accepted except after very prolonged investigation and discussion. Those he had described merely represented the best he had been able to devise.

From Mr. Lomax's remarks he was not quite sure as to whether he (Mr. Lomax) understood that the author only used air-distillation when topping an oil, the temperature of the oil being only raised to 260° C., and that the amount of the air circulating was very small, being only that contained in the flask and connecting tubes. He himself considered that no cracking action of any kind took place under these conditions, or at least that it was no greater than when steam distillation was used. This view, as he had already stated, was supported by the fact that the results of the tests he had obtained for the amount of kerosene and petrol on two different crude oils had closely agreed with those obtained by other competent observers who had tested the same oils using steam distillation.

Mr. Lomax further suspected that the results of his fractionations of the residual crude oils at higher temperatures gave evidence of cracking having taken place. He thought this suspicion was

unfounded, for in this distillation he had used steam, and no air, and the total loss was very small, whilst the distillates were remarkably light in colour. He felt that the American practice, to which Mr. Lomax called attention, of cracking oils intentionally in order to obtain more readily crystallisable paraffin wax, might conceivably be a correct procedure for the stills used, but using his form of still, the same, and probably a better result, could be obtained without any cracking taking place.

There was no difficulty whatever in carrying out or in making the necessary calculations when using the method he had described for determining the calorific value of motor-spirit in a bomb. The method was designed to avoid the dangers due to loss of spirit when weighing out, and also the danger of damage if the vapour of the spirit diffused in the oxygen in the bomb before ignition took place, for such a condition caused explosive combustion. Moreover the procedure of using a Lunge pipette, as proposed by Mr. Lomax, made it impossible to replace the air in the bomb with oxygen before igniting, and hence rendered it impossible to avoid a further source of inaccuracy when determining what he had called the "Water Value" of the fuel, a value usually large in motor spirits.

He ventured to think that when Mr. Lomax attempted to carry out the methods of determining the calorific value of motor spirits by the method he (Mr. Lomax) had indicated, in comparison with that which the author had used in routine-practice as described in his paper, he would quickly be converted to the use of the latter.

Mr. Evans stated that transformer-oils would oxidise, with the formation of water, when a current of air was passed through them. This might very possibly be the case under certain conditions of temperature, etc., but the author had contributed a paper to the Society of Chemical Industry on "The Determination of Minute Amounts of Moisture in Transformer Oils" of which an abstract appeared in the Journal of the Society, vol. xxxviii, 23 R (1919), and in this he showed that at temperatures up to 212° F., and with air bubbling through two gallons of dry oil at a rate of about three bubbles per second for a period of 1½ hours, no weighable amount of water could be obtained, and that therefore any oxidation of the oil which occurred under these conditions, was of an additive character, or was confined to the oxidation of the carbon, or lastly was so small in amount that under the stated conditions of test it was not detectable, and therefore did not in any way affect the investigation of the amount of water present in such oils. (*Vide* also *Ann. Rep. of Soc. Chem. Ind.*, vol. iii. Mineral Oils, p. 74.)

Further extensive investigations made by the author since the paper on the determination of minute quantities of water in transformer-oils was read have fully confirmed the conclusion originally arrived at as stated above.

With regard to the statement that when titrating the free organic acidity of oils, an addition of further amounts of the phenolphthalein indicator rendered it necessary to add more sodium-hydroxide solution, presumably to keep the solution at the pink tint which indicated free alkalinity, the writer could only suggest that the phenolphthalein solution itself possessed some free acidity. The phenolphthalein solution employed by the author consisted of 0·5 grammes of phenolphthalein dissolved in 70 cc. of alcohol, and diluted to 100 cc. by the addition of distilled water. This solution was then rendered distinctly pink in colour by the addition of N/10 caustic-soda solution. In titrating any solution, the volume of this indicator which was used was important, and the author employed 20 drops per 100 cc. of solution to be titrated; using excess of this solution caused the free acidity test to come out lower and not higher, but the error was quite negligible. The amount of free sodium hydroxide which could possibly ever be present in contact with an oil in the author's test for free acidity was so small that no saponification of the oil constituents could occur, but the proposal which Mr. Evans made to exhaust the oil with alcohol, and then to titrate the alcoholic extract was undoubtedly a step in the right direction. This method was now being tried at Portsmouth, but no conclusive results had yet been obtained. If it was finally shown that this procedure shortened the method at present used by the author, and that it was equally or more accurate, it would certainly be adopted.

The presence of water undoubtedly did cause very great variations between multiple check tests on the flash-point of an oil, and these became greater the higher the flash-point of the dry oil, at least up to a certain limit, and also the more water there was admixed with it. The difference of 120° F. in the determination of the flash-point of an oil of 327° F. was certainly considerably greater than any difference which the author had met with. The author agreed with Mr. Evans' view that the flash-point of an oil was raised by reason of the admixed water-vapour displacing the air, and preventing ignition, and also with the view that water by facilitating the evaporation of the oil by a species of steam distillation might conceivably lower the flash-point. It was to this view that he referred on p. 57 of his paper where he stated that there were theoretical grounds for considering that the presence of water might possibly cause the flash-point of an oil to be lowered.

The author's statement that the solubility of water in creosote-, petroleum- and shale-oils was probably less than 0·03 % could not be extended to other oils, in so far as he was aware, although it might very possibly be true also of other oils; he was not, however, in possession of any direct evidence on this point, and had not had the time to experiment upon oils other than those referred to. It was quite possible and even likely that the character of an oil did affect the amount of water which was necessary to saturate it, but the author considered that certainty could be arrived at only by experiments. These, it was believed, could now be carried out by means of the methods which he had described elsewhere.

He was much indebted to Mr. Evans for the reference to the paper by Groschuff, and would certainly take an early opportunity of consulting it.

Forty-First General Meeting.

This meeting was held at the House of the Royal Society of Arts, John Street, Adelphi, W.C. 2, on Tuesday, 18th November, 1919, Sir Frederick W. Black, K.C.B. (President) occupying the Chair.

The Members of Council present were Mr. H. Barringer, Prof. J. S. S. Brame, Sir John Cadman, Dr. A. E. Dunstan, Sir Charles Greenway, Bart., Sir Thomas H. Holland and Dr. W. R. Ormandy.

A message of regret for unavoidable absence was received from Mr. Arthur W. Eastlake, this being his first absence from an Institution meeting since he has had the pleasure of acting as Honorary Secretary.

There was a large attendance of visitors, amongst who were Sir Marcus Samuel, Bart., Admiral Sir William Pakenham, K.C.B., R.N., Sir G. G. Goodwin (Hon. Member), Sir E. H. Tennyson d'Eyncourt, K.C.B., and several ladies.

The President, in introducing the reader of the paper, Admiral Dumas, said he desired to quote a few words from Lord Fisher's recently-published book, to the effect that in turning over some of the notes he made when he was a young man, he came across what he evidently thought was a very choice piece of wisdom, namely: "Words spoken before a lecture muddle the subject; words spoken after a lecture weaken it." The author did not state whether his mature judgment approved of both parts of that dictum. For himself he thought it would be agreed that the first part was sound, but that the second was very unsound. It

was an unpardonable offence for a Chairman to forestall the subject matter of a lecture, but subsequent criticism, if it was of the right sort, i.e. from people who knew the subject, and spoke from a helpful point of view, should not weaken, but rather strengthen a lecture, bringing into relief its good points and eliminating its weak ones.

One of the main objects of the meetings of the Institution was to provoke discussion, and he thought the more broadsides or end-on fire those present could get into the author's ship, the better he would stand up to it, and that at the end of the meeting the Admiral would be found standing on the bridge with the flag flying, and with the inscrutable smile which a high authority had said used to puzzle the German Emperor in the days when Admiral Dumas was Naval Attaché in Berlin.

The following paper was then read :—

The Conservation of Oil.

By Philip Dumas, C.B., Rear-Admiral (retired).

About a week before his passing from amongst us, Sir Boverton Redwood invited me to give this Institution a lecture such as I had several times delivered in the Navy, upon oil-fuel : its sources, uses and conservation.

My acquaintance with the subject may be said to date from Lord Fisher's selecting me in 1912 as principal Secretary of the Royal Commission on Oil Fuels and Engines. Plunging then and there into the literature of the topic, I speedily realised its immense importance as a factor in the promotion of civilisation, but at the same time grasped the limitations of possible supply, the inefficiency of methods of search for further yield, and the immense waste of this splendid source of power through mismanagement in every producing field and in subsequent utilisation.

These impressions were corroborated during the progress of the Commission by masses of the most irrefutable evidence, which however I cannot divulge at the present time. Having extended my study of the matter in many books, in personal discussions with experienced men, and in direct connection with carriage, storage and use of oil on sea and land, for power and light, I venture to hope that my experience, opinions and deductions may be of some value even to a technical audience such as yourselves, with the sole reservation that I apologise in advance to all those experts whose knowledge in detail must, of course, be vast compared with mine.

In round figures the annual output of coal from the whole world to-day is some 1,250,000,000 tons, all of which is immediately available for fuel, is presumably used, and as there is some evidence that the quantity is falling, the effect should be to enhance the price.

In similarly round figures the annual output of crude oil is some 75,000,000 tons, of which at an optimistic figure 45,000,000 can be manufactured into oil fuel, and this also is used, and more wanted.

Now these are the two most important fuels for commercial purposes in the world to-day, and there being a far smaller amount, it is clear that the value of the oil fuel should be economically based on that of coal, but despite the fact that from one cause and another, to be detailed later, oil has many advantages over coal—and I shall be within the mark in saying it is three times as valuable—while coal has advanced in price some 250 per cent. in the last four years, there has only been an advance of some 100 per cent. in the price of oil.

It is almost needless to say that the consequent huge advantages of oil fuel has appealed to ship-owners and other users of power, with the result that all thoughts to-day are turning to oil, and it tends to be forgotten how limited is the quantity available, and that, unless the oil owners increase their resources or raise the price, the demand will very rapidly outrun the supply.

There is indeed, of oil fuel, only about one-thirtieth of the quantity of coal, and one sees at a glance that from its rarity alone it is a luxury, and as such, great efforts should be made to conserve it, and save every ounce that is possible.

It is to this fact that I wish to call attention to-day, and point out to the world at large that however used, oil fuel of any nature, and the by-products to be obtained in its manufacture, are from the comparatively limited supply, and their almost illimitable possible uses, LUXURIES; luxuries not only in the ordinary sense of the term, like diamonds and caviare, but luxuries leading to efficiency like Welsh coal or the splendid fresh brains of our young and imaginative thinkers, and so to be used only to the best advantage.

It must be quite clear to anyone who gives the subject a moment's thought that a wide consideration of oil and its by-products as very limited luxuries, has never been given at all, though there are signs of it in the U.S.A. to-day. Thus oil fuel is used, and will be far more widely so, by ships which don't need it, for engines where it shouldn't be, for the provision of power where an inferior substitute would do as well, and so on, and meanwhile there has not yet arisen in any country one single statesman who has even attempted to

check its absolutely free and wasteful use. Consequently I can't tell you with what pleasure I saw in the Spring that Lord Cowdray was attempting to force the hands of our Government to take some action in the matter of waste and conservation, though I need hardly say that so far I have seen no result.

It is this waste which is my key-note to-night, for enquiry shows WASTE—waste all along the line. Haphazard education and waste of brains, wasteful search, wasteful boring, wasteful production, wasteful storage, wasteful use of main and by-products, wasteful transport, wasteful retail sale, waste in the lack of concentrated chemical research into its constituents, and lastly waste in its most economic use. Indeed we might think that the whole oil industry had been nationalised.

I do not in the least want that any or all of these should be controlled by Government. Heaven forbid! The less Government interference in all matters of commerce the better, and having been a Government official myself I know what I'm talking about. But I do say, and I am certain, that well-considered reform should come from within the trade itself, and that proposals should be placed before the Government to decree that licences for boring within the confines of the British Empire should not be indiscriminately granted, except to well-found corporations with ample funds, that boring without considerable readily available storage should not be undertaken, and that it should be treated as a criminal act to burn or destroy what at the moment are deemed to be waste products, and which later chemical research may prove to be worth their weight in gold.

Furthermore and under the ægis of this powerful Institution—which should surely be designated Royal—experiments should take place with regard to the efficient burning of all sorts of oils and better forms of burners and methods of burning obtained. For instance, by very careful attention to detail in supply and burning, I know personally of a case where a saving of 16 per cent. of oil fuel accrued for the same results within a fortnight. Any good housewife can tell one of wasteful or efficient lamps, or any good motor engineer of efficient or inefficient engines. In fact no one should be permitted nowadays to waste this unequalled form of power which Nature has placed in our hands; and incidentally some form of licence to burn or use based on efficiency might well prove a real source of profit to Governments.

Dealing with the points of my lecture *seriatim* and as shortly as possible, I have spoken of haphazard education. Is not that a fact? Have we instituted proper chairs of geology with regard to

the future location of oil at our Universities. Why there's ample room for a university for geological education alone and surely the great Companies should build and endorse one. It need not be confined to oil alone. Gold, Diamond, Coal, Copper, and many other great mining companies should join in its institution, and, judging by the dividends they pay, there must be ample funds available. Believe me, until such a University is started, there will be waste of brains, and waste of brains in the long run spells waste of efficiency and dividends. Scholarships also, as appealing to a larger circle, should be instituted, and it's up to you to provide them!

Until a concentrated effort of this sort is made in Great Britain, or better still the British Empire, we shall be subject to the mortifying experience, as during last Winter, of American experts coming to locate oil in England. For Heaven's sake, let us keep our fees in our own country, and carry out with our own resources the location and boring for oil. As regards this I regret to say I am hardly qualified to speak, my information being wholly obtained from reading, but it is surely common knowledge that frightful waste in inaccurate and unsound location and wild cat boring has taken place in the past, which might have been obviated by wider knowledge.

In this matter it would seem reform is necessary unless, as I hope, the whole oil industry falls into the hands of a few great well-managed monopolies. I mean commercial monopolies, for Heaven preserve us from Government monopolies, always wasteful and inefficient.

As regards production again, has not frightful waste taken place owing to the lack of immediately available storage and has it stopped to-day? I really don't know, but it would be instructive to learn if proper tanks were ready *before* the flow of oil from the borings in Derbyshire and other places during this year. In my mind I doubt it, and, from my reading, the same lack has obtained everywhere. That isn't sound, and as oil, or any other fuel for that matter, is indirectly the eventual property of the public, we have some right to insist that it shouldn't be wasted. From the perusal of a clever and thoughtful lecture by Mr. J. W. Burford, given here in May, 1918, it would appear also that the machinery in use on the oil fields of to-day is notoriously inefficient, and has often been placed without due consideration of local possibilities. Of course as long as good dividends are paid, shareholders are satisfied, but later methods would tend to increase and improve production, and as the shares rise in value, and become the property

of a wider circle of holders, such increase and improvement will be called for, and more important still, the oil will last longer.

The waste in refining processes is much reduced by continual improvements, but there is still room for its further reduction, the recovery of more of the valuable ingredients, or the diminished injury from errors in treatment.

As regards storage, apart from direct leakage, the inadequacy in size of tanks, their inconvenient situations and inefficient pumping installations are all points involving uneconomic waste of time, labour and material.

The extreme variety of forms in tank-ships suggests that the most efficient type in respect of size, shape and speed has yet to be decided upon. As to the nooks and crannies, whether double-bottoms or otherwise, final extraction of the oil from them is possible only by a cleansing that destroys the lingering portions.

In transport for sale, it would be interesting to know what percentage of kerosene, petrol, or lubricating oil remains in the "empty" drums or tins—not to speak of deliberate pouring away of unused oil.

In respect of wastage in transit the amount seems to be small —as I was told only 1 per cent.—but even that is probably unnecessary, and 1 per cent. of a 12,000 ton cargo is by no means to be sneezed at, and could be reduced by attention to detail. In the transport for retail trade, surely for the lighter oils at all events small pipe lines diverging to great distributing centres would be sounder policy than horse or motor transport of great boiler-like drums, and of innumerable petrol-tins in carts. Of course it would be difficult, owing to the emanation of vapour, but such difficulties are soon got over by the chemist and engineer in collaboration: The gas Companies can manage transmission over many miles.

As regards wasteful retail use, it seems to me that a licence to burn oil in any engine might well be instituted, and bear some relation to the economical efficiency of the engine, so that if a man chooses to purchase and use an engine which is not efficient, he should be to some extent penalised by the cost of his licence as well as in the wasteful loss of economic power.

Coming to chemical research into the constituents of oil fuel, I am perhaps on dangerous ground, and I have noted with pride and satisfaction that some of the wealthy oil Companies are endowing research at Cambridge, but is that sufficient, and do we do enough in this way in Great Britain? It is usually thought of course that Germans are pre-eminent in this matter, but equally of course it is not wholly true, and from a lengthy residence in that

country as Naval attaché, and thirty years continued knowledge of it and the Continent generally, I am sure I know my facts. What does happen there is that in any works large numbers of young and imaginative (some Germans have imagination) chemists are employed, and working under one chief carry out research in various directions far more speedily than our usual meagre staff of chemists can do. For instance, in similar departments of Krupp's and a great munition-works in Great Britain there were employed 120 and 7 chemists respectively. In each case the chief was probably equally clever, but no man, however able—and I believe the Englishman was the better man in this as in all other things—can hope with only six assistants to cope in results with one who has 119.

It was, I believe, the work of a similar great staff in some oil-works outside Great Britain, which disclosed to the owners the abundant presence of toluol in certain oils, which immensely important fact was brought to the notice of our authorities by the patriotism of Sir Marcus Samuel. It is interesting now to reflect that it was largely this fact that enabled us so greatly to increase our output of high explosives in the early days of the war, and as the information was first communicated through myself, and as it directly helped my own especial requirements, and saved me many sleepless nights, I can't help mentioning it, as honour has unquestionably not gone to whom honour is due. But that is always our way.

Where toluol exists in secret, who yet knows or realises how many other valuable factors the sun or something may not have stored up for us in oil. Why we don't know yet where oil came from or how it was made, or what oiliness is; nor can we apparently refine to an oil comparable with sperm. Why do we not let imagination have its sway, and provide the humble means, chemists, young imaginative chemists, to disclose what has been laid up in store for us. This surely is a matter for this great Institute to take in hand. Do please look into the matter. You will presently deserve the thanks of your fellow men. A Chair of Chemistry for Oil Research at Cambridge is, I am certain, quite insufficient, and if a University for Mining and Chemical Research in connection therewith cannot be started, endow a similar Chair at every University. Why the cost would be infinitesimal compared with the possible benefits that will accrue, and if oil were but raised to its proper economic price, there would be millions available.

With these general preliminary remarks I would like to turn to the—at present—greatest users, and probably wasters of oil fuel.

Undoubtedly it is wasted when burnt under a boiler to raise steam, and the worst sinners in this respect are warships, and their officers, but unfortunately the reasons for and advantages of the use of oil fuel in ships in preference to coal far outweigh the disadvantages, and as many of them apply equally or with greater force to merchant-shipping, it is perhaps worth while to detail them. They are :—

ADVANTAGES.

(1) Rapidity of raising steam.

(2) Oil alone gives an excess of speed over coal or coal and oil in combination.

(3) For the same weight of fuel the distance covered is increased somewhere about 50 per cent. when using oil.

(4) Oil can be more easily stowed and more—say 40 per cent.—carried ; also it can be stowed in double bottoms, but this is not wholly an advantage, as it gets into pockets and can't well be extracted.

(5) Generally speaking the centre of gravity is lowered and a greater choice of cargo given thereby.

(6) Greater economy in fuel ensues while waiting. There is a constant readiness to sail and demurrage is saved.

(7) A great saving in shipping in replenishing depôts ; also much less handling, and wages bill less.

(8) Ships can re-fuel quicker and more easily, and even in open harbours or in a heavy sea.

(9) Bunkers can be replenished without undue labour.

(10) Constant facility in delivering fuel to furnaces, and no trimming bunkers.

(9 and 10) These are matters of huge importance in view of the general movement in the direction of lesser hours of labour, and the arduous work of coaling and trimming bunkers.

(11) Very little or no cleaning of furnaces necessary : which is a great factor in long-distance runs.

(12) A general reduction of stokehold staffs by some 60 per cent., a very great advantage nowadays.

(13) A generally lessened cost of up-keep ; as regards which it is difficult to arrive at figures, but probably more than halved.

(14) There is no deterioration in keeping oil.

(15) The comparative cheapness of oil (though this can't possibly last).

(16) The widespread use of oil will inevitably lead to the internal-combustion engine on a large scale.

DISADVANTAGES.

(1) The present limited supply in Great Britain and the British Empire and lack of re-fuelling facilities on certain routes. With regard to this I understood that the Petroleum Research Department carried out deep and invaluable investigations during the war, and gave incisive advice as regards increasing the supply available in Great Britain. For some unknown reason the matter seems to have dropped, which looks like normal Government ineptitude, and one would like to know why. Will Governments never learn that their researches are for the public benefit? Surely the matter should be re-opened at once in the Press, and in the House of Commons, for it is clear that the public at large don't realise the possibilities from cannel coal.

(2) Danger of shortness of supply at any moment and at any place; though commerce will probably take this in hand.

(3) Crude oil is to some extent a monopoly of certain countries, and its export may always be taxed by Governments, which, with their present ship-building policy, is at least likely in the U.S.A. We should therefore rapidly develop the resources of the British Empire. Again as regards this, I know there was much valuable work done by the Petroleum Research Department at the Admiralty, and its publication should be insisted upon by the Council of this Institution.

(4) Oil fuel is a manufactured article, and cannot always be used in a raw state like coal.

(5) Some oils freeze at low temperatures, requiring heaters to enable delivery to furnaces.

(6) Dangers from fire on board, though these are not serious.

(7) Dangers when stored ashore, but these are not great.

(8) Wicked waste of economic efficiency in burning oil for steam-raising under a boiler as compared with its use in an internal-combustion engine.

As a broad result of all these advantages and disadvantages, it may be stated with confidence that:—

(1) For steam-raising under boilers, oil is to coal as 3 to 2 at least.

(2) For internal-combustion engines, oil is to coal as 4 to 1 or, compared with the latest geared turbine, as about $2\frac{1}{2}$ to 1, and, personally, I believe these proportions, when everything is taken into account to be still understated, but, even if they aren't, it is certain that the advantages, whether for warships or commercial purposes, far outweigh the disadvantages, and the only real trouble is possible shortness of supplies.

On the other hand we do know that very satisfactory results have been obtained from coal, and the broad question is whether we are justified in thoughtlessly utilising an immense luxury, which strangely enough is a cheap luxury.

Now, Gentlemen, we are or were luxurious people in the Navy, and insisted—and rightly so, as results have shown—on having the best for war. Moreover I am prepared to confess that whether in peace or war we got it, though that is largely due to the amazing ignorance of Ministers and the House of Commons, who never can intelligently criticise our demands, because, in fact, they don't comprehend, and are not as a rule told, what we are talking about.

That occurred with oil fuel. As is public knowledge, the Royal Commission guardedly recommended its general adoption. The Navy got an inch, took an ell, and no one demurred, and I need hardly say liked it, and to-day for all practical purposes the restrictions have been dropped, and we use and burn and waste oil equally in peace and war.

In Germany, prior to the war, where the Minister of Marine—an old friend of mine called Tirpitz, and a very able man—was a naval officer, and had to explain things in the Reichstag, there was of course far more intelligent criticism, and I well recollect his remarking sadly to me that oil fuel was a luxury to be supplied and used in war alone—where waste must happen—or for occasional practice in peace time. And similarly in the Dutch Navy, where the Minister was also a naval officer, their ships were only permitted to use it in the East Indies.

With some such form of restriction I am in agreement, and especially to-day, when for all practical purposes in my opinion large battleships and battle cruisers have had their day and should cease to be. But if they must be retained to minister to the vanity of nations, let them move about burning coal until we can provide them, if further reflection should demonstrate their value, with internal-combustion engines. The case is different with submarines, destroyers, and small craft. These must go on and retain liquid fuel, but except in the case of the first-named, coal and oil can be used, and only coal in peace. Indeed the coal as a means of protection may be an advantage.

As I said earlier the days of solid fuel for shipping and big boilers ashore are over. Stokers will not work these huge amounts with heavy manual labour when once they have become accustomed to turning on a tap. Think of what it means in a huge mail steamer to deliver 500 tons of heavy, dirty coal to furnaces per day *by hand*, and also to turn forward a similar amount in the bunkers amid

darkness, dirt and heat. Everything points to labour refusing such a task.

Moreover, with the surcease of solid fuel, huge sums will be saved in demurrage, in labour and in cleaning, so that, as an able merchant-service friend of mine has computed, that for large mail steamers whose job is to deliver passengers and goods, re-fuel, re-load and return, oil would be six times as valuable as coal.

The questions are, firstly, would it pay to convert boilers and bunkers to burn and stow liquid fuel, and, secondly, can this be got from coal?

As regards the first point, my information as regards a large mail steamer lately so converted is that the cost was horrifying—some £600,000—and yet it is expected to pay: and as regards the second, that it can be done at the pit's mouth, where moreover the small coal is equally valuable with the large.

It is certain that liquid fuel engines will be in use everywhere, and coal must be converted into electricity and liquid fuel at the pit's mouth. Anyhow, a public assurance on this question is an absolute necessity.

Do please realise, Gentlemen, that oil is required everywhere for the economic commercial rehabilitation of the world, and it is up to you to check naval extravagance in this matter. Left to themselves and the sentimental lack of common sense, and equal lack of knowledge of the House of Commons, be sure that our Navy and those indeed of other countries will be run on oil, and a splendidly efficient natural product wasted. Oil is a factor as regards navies for *war*, and the supply for that for the first year or so should be stored and kept in the country in commercial hands, but in peace time insist on navies burning coal. There are it is true difficulties in connection with this, but by no means so great that they cannot be got over.

If anything is certain it is that deep thought must shortly be given as to the possible affording or retention at all of great and costly battleships costing £6,000,000 or more each, and whether smaller vessels of the submersible type for use on sea or land, may not shortly take their place. It would be easy to design such a type with the advent of the internal combustion engine, and I shall be astonished if we don't see them before long. Press the matter, and at any rate save our present wild expenditure of oil.

As regards these statements of mine it must further be remembered that although speed is a great factor, it is not of the same value or necessity for all vessels. Many—probably 60 per cent. of the war vessels of any country—can be run on coal with no loss of efficiency

whatever. Ardent young naval officers will of course tell you it is not so, but don't believe them, and bring pressure to bear on the Admiralty and the House of Commons, or by far the greater portion of the oil-supplies of the world will be required by them. You will have greater difficulty, because oil is cheap while coal is dear, and I need hardly remark in this room that the Director of Naval Contracts is always a very shrewd and clever man.

Save the oil, conserve it, take care of and preserve it. If for nothing else we shall require it for the wars of the future, which believe me will come, despite the efforts of the late army of geniuses in Paris to make war upon war, and future wars impossible. Does anyone believe that war has come to an end? Can anyone really believe in continued peace who has made any sort of a study of International problems? Why there are materials for a dozen wars in the present peace terms, and the late war has been fought on and won by oil from first to last, whether on or under sea, in the air or in the trenches, and that country which has a reserve of it (thank God! Germany hasn't) will again win in the future. You can do much to check its extravagant use in ships of war meanwhile, and it is urgently necessary to do so.

Again also in Merchant Shipping. It is true that but little oil fuel is used here at present, but if it comes, and the wise economic working of fast liners, cargo and mail boat would seem to demand and make it certain on particular routes, then insist on its most economic use, and as a first step raise the price of oil to its proper and legitimate height. With practice and rough-and-ready trials and experiments, the Navy has learned a great deal of how to burn it to the best advantage, and much of this knowledge could, and indeed should, be made available for public use. Indeed now we are at peace, with no immediate prospect of a sea war, there is an immense amount of naval experience which might well be at the disposal of the mercantile marine. As I hinted in the earlier part of this lecture, there is still a vast field of research in economy in burning oil, and always (as we used to say in the Navy) 80 per cent. goes up the funnel. And it needn't, and it shouldn't. Believe me, we of this generation will be cursed by our successors for the wicked way in which we have wasted both coal and oil, and as regards the former very largely because it could have been turned into liquid fuel with economic advantage.

And remember that the United States are at last making real efforts to create a mercantile marine, and many of the ships under construction are to burn oil. Similarly to the British Navy they will like it, and extend its use, and the prospect of any much free oil

for export from the U.S.A. is not rosy unless the strictest economy prevails, while practically all vessels using the Panama Canal route will go to oil, and use up much of the Californian and Mexican supply.

From every point of view then, it behoves us to economise in the use of oil, for at present from price alone the manufacture of liquid fuel from coal in the British Isles is almost prohibitive. Furthermore, every ounce of coal above that which can be soundly utilised at the pit's mouth for transformation into electricity, will be required not only for our own mercantile marine, blast furnaces, domestic use and so on, but the residue will be required for air, road and water motor-transport, of which even the least observant must have learnt the value during the war.

It seems to me that anyone with a grain of imagination can foresee motor-launches, motor-lighters and motor-lorries at work in tens of thousands before long, and at the same time liquid fuel will be required for shipping, oil-engines and the huge number of motor-cars and aeroplanes which but a few years will see in the possession of everyone.

Think of what it means that the Ford Company alone are turning out 2,000 or 3,000 motor-cars a day, and that we can put 15,000 motor-lorries on the roads. All must eat up some form of oil fuel until or unless our hopeless authorities will realise the possibilities of alcohol. Government or any other cars and lorries don't save oil, you know, and all will certainly be wasting a portion of it, and lubricating oil as well. The waste of lubricating oil is quite frightful, and I believe that any two first-class chemists and engineers who took up the subject in collaboration could presently point out how to save at least 30 per cent. of all the lubricating oils wherever used. What a field for research and what a fortune waiting for a young man. Why the roads of almost any country to-day are saturated with lubricating oil, and you can trace the path of most ships on the sea by the film of wasted oil which they leave behind them.

Consider the motor-car engine. How much fuel is absolutely wasted in it by faulty design, and how much by uneconomical selection of fuel. In many cases any substitute would serve, and the fortunate owners of crude oil need fear no competition: they will presently be called upon for twenty times as much as they can raise, and at their own prices.

Look, again, at the fearful waste in connection with aeroplanes, where weight-carrying capacity seems to be the only consideration. Unless the super-chemist of the proximate future can furnish us with synthetic oil from terrestrial or marine sources, the coming millions of aircraft will be inert from lack of motive force.

Meanwhile and for the near future the value of oil for the advance of civilisation and the increase of production, is such that the most minute and systematic search should be made for it everywhere. Are we making such a really systematic study of the sources of crude oil in the world, or indeed training and employing a sufficiency of geological experts to carry out the work? I fear the answer can be but no, and yet there are thousands of able and adventurous young men in England with brains to be trained and used, and you should do it. Can anyone doubt that there are vast fields of oil yet to be located? Why twice in my life, once in the Indian Ocean, and again on the west coast of Africa, I have steamed through oil, and the atmosphere reeked of it. It is up to the great and wealthy Companies (thank God for competitive monopolies and capitalists!) to search for and locate the sources.

And we know of and suspect vast deposits of shales—hundreds of thousands, probably millions of square miles of such deposits of great depth: indeed an enormous and overwhelming source of power to be tapped and used—and used economically. Are the great oil Companies preparing for the inevitable future exhaustion of their fields by purchasing these, and carrying out systematic research in good time into the most economic method of extraction of the oil and its use? Of course I know that a great deal has been done in this direction, and even now Companies are exploiting the shale fields of Great Britain in many localities, but one wants it on a larger scale, and above all to increase our production at home. The Dorset shale we know produces good and useful oil which, with some of the sulphur extracted can be mixed with purer types, and so used to advantage. This source should certainly have been brought into use during the war.

What has happened as regards the Dorsetshire shales? Why I remember being told by Sir Boverton Redwood in the very early days of the war that by some process the greater proportion of the sulphur could be cheaply removed, and for many a day I was happy in the hope that they would be exploited. And apparently they haven't been. Wake up, oh ye capitalists! Millions have been subscribed for developing what I believe to be inferior shale-fields in Norfolk and other places, and why not here also?

Do please bear in mind that the experts give all existing fields a life of but 40 to 50 years at most, and although we all know that experts are not infallible, yet there must be an end of it with the present wildly extravagant use of oil fuel, and above all, if it is sought for, extracted, used and wasted in so mad a manner as in the United States in the past, one can't hope for a very long period of produc-

tion. Shortly, very shortly, the owners of all ships and engines will realise its efficiency and present cheapness, the cry will arise for it everywhere, and there's only one-twentieth of our requirements. So again I say put your army of experts—a very small army at present I'm afraid—on to systematic research as regards shale, and it will prove well worth your while. Indeed any system of production of oil will unhappily be eventually well paid for by war. This war has been largely waged on oil. The next one will be nearly wholly so, and Bismarck's dictum of "Blood and iron" brought up-to-date would read "Blood and oil." Therefore yet once again I say: Find and conserve your oil, and in so doing it may well prove that you will preserve your country—this England, which, with a heart of oak and set in a silver sea, quite naturally requires something of the creosote type to keep it sound.

And lastly, when next you go into the streets look for, and you will probably curse, a motor bicycle. But think over it, and you will presently realise what a beautiful creation it really is. Truly a triumph of the engineer and the chemist. And the inwardness of it? Surely the beautiful little internal-combustion engine with so much power concentrated in so small a space and so efficient, and yet even that we all know could be mechanically improved by the substitution of the turbine form for the reciprocating type of engine.

Here indeed is the type of engine of the future, and is it really true that we can't get the internal-combustion engine on a large scale? Literally I don't believe it, and I beg you not to listen to the despairing tones of our experts who say it is impossible. Remember that the very fact of being an expert means gain in experience, with a loss in power of imagination, that few are young men, to whom the future belongs. Further let your memories revert to that very instructive sermon preached from the stage a few years ago, and called "Milestones."

Insist everywhere, regardless of ridicule, upon the urgent necessity of this development of the embryonic engine of the present. Prophesy sufficiently, and the prediction will be verified: an inventor will arise: one comparable with Watts, and a greater than Sir Charles Parsons, and incidentally he will command what I saw called the other day a Rockefeller fortune. Not till then will cease the abominable scandal of wasting precious oil by burning it under boilers. Largely owing to the lack of a suitable internal-combustion engine for merchant shipping, I compute that roughly a hundred million sterling per year is wasted, and something like a half of all the oil produced in the world is clean

thrown away. Of course it would be easy to produce figures in support of what I say, but no one listens to or understands them, and indeed it's wise, considering how easy it is to manipulate them.

But indeed it is true beyond cavil that the advent of the internal-combustion engine will save vast sums, and when one thinks over the waste of value, power and efficiency in burning oil to raise steam, it should reduce all the happy owners of oil to tears; that is unless they are ignorant of the fact that their fortunes are being thrown away. To you who do understand I would say that in the intervals of weeping you should constantly ponder over the realisation that with its arrival on a really large scale, the life of your oil fields, and incidentally your fortunes may well extend to the neighbourhood of another hundred years.

With that pondering will come the determination to forward the development of the internal-combustion engine, but do let that, as with all other great advances, come from Great Britain. I am told that the big experiments are still in the hands of the Germans and other foreigners. Now I know Germany and the Germans through and through, and, man for man, you are better than they are in geology, chemistry, and engineering in just as great a degree as you proved to be in fighting. Realise that fact. Away with the Germans! and endow and employ young Englishmen—or as it's a case of an engine perhaps I had better say Scotchmen—to devise and develop this great want of the world.

Recollect that there is no time to lose. Already the Germans, Dutch and Danes have had a measure of success, and meanwhile there is no actually all-British internal-combustion engine on the market at all. And yet you have in England to-day—I hope in this room—probably the greatest living expert on the subject, whose brains and experience are worth millions to anyone or more of you who is wise enough to catch him at once. When you leave here to-day I beg of you to try and find out what have been the profits of that admirable firm called Burmeister & Wain in the past few years, and form a similar British firm, and make your own millions. If I had any capital—but for thirty-seven years I was simply a naval officer, so of course I have none—I would make them myself. Believe me they are just waiting for the first of you who is wise enough to pick them up.

Do let us try to get into motion. Until then, we shall see an ever-increasing wicked and avoidable waste, and I beg and implore of you to stop it, and the equally true fact that the oil industry is tinged all along the line with waste: abominable, avoidable waste.

Of course the same thing may be said of coal, but there we have

what we in our little finite minds choose to consider as an illimitable supply, which is comparatively true with coal, but it is not so with oil, and in view of the certain future vast requirements and the inevitable arrival of the internal-combustion engine be sure of this, that we shall require all the oil we can possibly produce or save.

Look at it how you will, the supply is limited, and the requirements enormous—as I said earlier, at least twenty-fold—and we have not got it. Meanwhile, as I have tried to point out, we see waste everywhere of what, bar wheat, is probably the most valuable natural product of the world. Much of that waste is avoidable, and it's up to you, Gentlemen, and your Institution to check it, and conserve for the greater efficiency, civilisation, and comfort of the world at large this splendid gift which the Almighty has placed in your hands.

In conclusion, I must apologise for rushing in where the angels appear to be fearful to tread. I can only say that that great and wise man Sir Boverton Redwood, who knew me well, advised me to offer such a lecture to you. If I have trodden on some toes, or hurt anyone's feelings, please remember that I am a sailor, and that to blunt honesty much may be forgiven. At any rate I represent no commercial interest whatever, I am unbiassed, and have no fish to fry beyond a desire to stimulate thought, and render what little aid I can towards the interests of the British Empire in general by the conservation of oil.

DISCUSSION.

Sir Marcus Samuel, Bt., in opening the discussion, said the author had made the statement that Lord Fisher knew nothing about liquid fuel. He desired to assure Admiral Dumas that as far back as 1900 Lord Fisher told him that he was a fanatic on the matter of liquid fuel; and he believed it was common knowledge that had it not been for Lord Fisher's fanaticism, and the pressure he brought to bear in the appointment of at least one, or he thought two, Commissions, liquid fuel would never have been introduced into the Navy. It had been introduced into the Navy, and in spite of the great waste in burning it, he did not suppose there was a single expert in the world who would revert to the use of coal. He had always unhesitatingly said that there would be sufficient oil forthcoming to meet the demands, and time had proved the correctness of that statement. It must be of great interest to the Chairman and everyone interested in oil to know that within the last ten days three wells had been successfully

brought into bearing on a virgin field, which yielded between them the enormous amount of 30,000 tons a day, so that he did not envisage any great danger of lack of liquid fuel in the future, although concurring in every word the author said, that it should be conserved in every possible way.

Conservation would be furthered above all by the construction of internal-combustion engines, because in that respect the greatest economy was possible. It was not only that the internal-combustion engine was far more economical in its consumption of oil than other engines, but there was no waste in using it. The moment the engine stopped the consumption of fuel ended, and it was ready at a second's notice when it was wanted again. He was very glad to be able to tell Admiral Dumas that he was mistaken in supposing that foreigners had had any monopoly in the development of the industry as it was to-day, although such might perhaps have been the case in regard to its creation. A very great many British firms had taken the matter up *con amore*, because they saw at long last that it was inevitable. Lord Pirrie had told him that Harland & Wolff were turning out a set of internal-combustion engines every six weeks. In a small way he (Sir Marcus) had done what he could to further that development by giving orders for experimental engines to two of the greatest engineering firms in the world—the Wallsend Slipway Company, who made the engines for Messrs. Armstrong, Whitworth & Co., and Swan & Hunter, whose engines were made by the Neptune Works. It was one of the minor tragedies of the war that the three ships which they built were all torpedoed by the Germans, but they existed long enough to prove the complete success of the English-built vessel. The experience which was obtained from the running of those boats survived, and it was quite sufficient to demonstrate that there were no insuperable difficulties in the internal-combustion engine, and that the faith they had in British brains was more than justified. He was quite certain that in time the finest internal-combustion engine that could possibly be constructed would be evolved.

It was with the greatest possible pleasure that he moved a vote of thanks to Admiral Dumas for the very interesting paper he had read, and for the breezy manner in which he had read it. He did not know whether motorists always confined themselves exclusively to bonnets; he had seen them investigating under hats too!

Motoring was certainly one of the wonders of the world, but he often wondered whether motorists ever realised that in their every-day evolutions motors were running with a spirit which came from eight or ten thousand miles away, and which was stored in the

bosom of the earth until it was needed for human want. The question was an exceedingly interesting one, and the members thanked Admiral Dumas most heartily for the excellent way in which he had put it before the meeting.

Admiral Sir George Goodwin, K.C.B., LL.D. (Engineer-in-Chief of the Fleet), in seconding the motion, said it was some months ago since the Institution did him the honour of electing him as an Honorary Member, of which he felt very proud, but up to the present he had not had an opportunity of addressing the members. He was extremely glad to find that, on the first occasion on which he did so, he had the opportunity of seconding the vote of thanks to his old friend Admiral Dumas.

The subject to which the author had called attention was a very important one, and he seemed to have paid a great deal of attention to the Navy in dealing with it, although very little that the author said was to its credit. Such fearful things having been said about the burning of oil in the Navy, he was sure he would be pardoned if he made a few remarks, in which he would not be able to agree with the statements that had been made by the author.

First of all, referring to a matter of detail, the author said : " As we say in the Navy, we send 80 % up the funnel." He did not know where that was said in the Navy ; he thought the Admiral must have got mixed—he meant the odd 20 %. Then the Admiral went on to say that ships could be traced on the ocean by the lubricating oil that they left in their track, and he held out the possibility of a big fortune to some young fellow who would come along and introduce economies. He had called for some figures in regard to what that dreadful waste actually was, and found that the actual expenditure of lubricating oil in their battleships was 14 gallons a day, in the light cruisers 5 gallons a day, and in the destroyers under 1 gallon a day. He mentioned that because he thought the Admiral should not go much further in offering fortunes for introducing economies in that respect.

The author suggested that the Admiralty should be forbidden in peace time to use oil in the boilers of the battleships and the destroyers, and that coal only should be used. He believed it was well known that that matter had been fully considered at the Admiralty, and that a great deal had been done in regard to it. No more oil was burned on the battleships than was necessary, but if they were not to burn oil in the destroyers there would be no such vessels at all. They were designed for burning oil entirely, and it was impossible to go back to coal. A new type of ship altogether would be required if coal had to be used, and they would

be of no use as destroyers in war time. As an ideal it was quite right to say that oil should not be used in peace time, but it was absurd to say that it was abominable wickedness to burn oil under those circumstances. There was nothing else to be done, and he said that with all the authority of the position which he occupied.

With regard to the question of internal-combustion engines taking the place of steam turbines in warships, the machinery of the biggest battle cruisers developed 144,000 h.p., while the machinery in the present destroyers developed 28,000 h.p. He believed the biggest horse-power that had been obtained from the cylinder of an internal-combustion engine in any installation fitted afloat was in the region of 400 h.p. (**Sir Marcus Samuel,** interposing, said he knew of a case in which 750 h.p. was now obtained.) **Admiral Goodwin,** continuing, said that accepting the figure of 750, if that was distributed over 144,000 the total number of cylinders required in the present battle cruisers would be obtained, and by dividing 28,000 by 750 the number required in the destroyers would also be obtained; and he invited anyone to send in a plan to the Admiralty for driving four shafts in the one case, and two shafts in the other case, with that number of cylinders suitably disposed. Further than that, the machinery, including boilers and all accessories, in a destroyer weighed 30-35 lbs. per h.p., while the machinery in the ships which were now running on the ocean for commercial purposes weighed in the region of 280 lbs. per h.p., so that the internal-combustion engine of the merchant service, with its accessories, weighed about eight times as much as the destroyer engines with their boilers and all accessories. Much lighter internal-combustion engines, running at high speed, were in use, but their horse-power per cylinder was generally much less, and even these engines weighed over 70 lbs. per h.p. That showed that it was impossible to put internal-combustion engines at present in the destroyers; it was impracticable simply on the score of weight. He did not say it was impossible, but the Admiralty was not ready for it yet, and a great deal more work would have to be done on the subject before it could be said that it was economical to replace steam machinery of high power by internal-combustion engines.

Then it was said that the Navy should have used home-produced oils. No one would have been more delighted than the Admiralty if they could have used home-produced oils in addition to shale-oil during the war. Several such oils were brought to their notice, but not one was suitable by itself for use as an Admiralty fuel, and he thought the members of the Institution should find one before the

Admiralty were told that they should have used home-produced oil. Some home-produced oils could be used as mixtures with other oils, but very few of them. Further, the quantity of oil that could be obtained from the whole country was only about one-tenth of the amount required by the Admiralty; and after careful thought it was decided that they could not incur the inconvenience and the possibility of injuring suitable oils from other sources by mixing the home products with them.

Sir E. H. Tennyson-d'Eyncourt, K.C.B., said that while he felt the audience was very much indebted to Admiral Dumas for his most interesting lecture, like Sir George Goodwin, he could not agree with all the author said in it. He thought the author had been rather down on the Admiralty, where till recently he had held an important post.

He could not agree for one moment with the suggestion the author had made that all battleships ought to be done away with. That might possibly come in the future, but he did not think the time had yet arrived.

He also desired fully to indorse the remarks Admiral Goodwin had made in regard to the huge advantages of oil fuel for warships. He had omitted, however, to mention the fact that as much as 9,000 h.p. was obtained from one boiler by the use of oil in some of His Majesty's ships, a power which it was quite impossible to obtain with coal under one boiler, and that was a very great advantage in warship design, especially that of destroyers. He agreed with the author that it would be a tremendous advantage if an improved internal-combustion engine could be made with enough power in the cylinders, the weight being reduced to something approximating to that obtaining in aeroplane engines, but this would take an immense amount of development.

Admiral Sir William Pakenham said he had the great privilege of being at the Admiralty when the use of oil in warships was in its infancy. Sir Marcus Samuel was continuously at the door of the Admiralty, never giving them a moment's rest, and insisting that attention should be given to the subject of the use of oil in warships. Sir Marcus said at the time that he was prepared to combat every doubt that existed in their minds on the subject.

He had thus rendered to the country a stupendous service, and probably was mainly responsible for the fact that even the first ships turned out after the declaration of war were oil burners. He desired to join in the heartiest manner in the vote of thanks that had been moved to Admiral Dumas for his most interesting lecture.

Sir John Cadman (Past President) said he had very little to say

except join in the vote of thanks which had been moved to the Admiral for his very useful contribution. This country had won the war, and it was the particular type of individual embodied in the author that accomplished that fact.

The author had described in a very breezy manner the waste associated with the use of petroleum. He would, however, draw attention to the waste in the use of coal as compared with oil so far as their respective calorific values were concerned. It would then be found that the technologists responsible for the use of oil had made much greater progress than those concerned with the use of coal. He hoped the author would in another place give a similar discourse on the great waste of coal.

Sir Charles Greenway, Bart. (Past President) said he desired to join in the vote of thanks to the author for his very animated and picturesque paper, more particularly because he thought it would have the effect of stimulating all the members to take a greater interest in the promotion of the study of oil technology. The ground covered by the paper was so vast that it was impossible for the meeting to deal in a suitable way with all the points raised, but there were a few points to which he wished to refer.

First of all the author was very pessimistic in regard to the production of oil in the future. He pointed out that the present production of the world was something like 75,000,000 tons per annum, compared with 1,250 million tons of coal. He presumed everyone recognised that the production of any commodity depended to a great extent upon the demand, and now that a demand had arisen for oil as a result of the additional experience and knowledge gained during the war as to its direct economic advantage and utility, he thought they could with perfect confidence look forward to a corresponding increase in the production. He had not the least doubt in his own mind that, provided remunerative outlets could be found for such a huge quantity of oil, the quantity which would be produced in the world in the next ten or fifteen years would be fully doubled. The quantity then produced would, according to Admiral Dumas's figure, be 150 million tons per annum, but, as had already been pointed out, the comparative efficiency of oil to coal was 3 to 1, which meant a comparison of 450,000,000 tons of oil against the present production of 1,250,000,000 tons of coal. That could not be regarded as a very insignificant quantity for the uses that were likely to develop for oil.

There was one other point in the paper that attracted his notice and that made him rub his eyes and wonder whether the paper had not been written twenty years ago, namely, the author's references

to the necessity for oil Companies to carry out experiments in the most efficient use of burners, the necessity for further chemical research, and so on. The oil Companies of the present day must have hidden their lights under a very considerable bushel, otherwise the author would have known that the big oil Companies of the world had been doing all that he said they ought to do for years past. Each Company maintained a perfect army of chemists, and his own Company alone possessed a staff of chemists that would compare with any German concern, both in regard to research work, and in regard to getting the best possible results from refining, and he was quite sure that none of the other big Companies were behind in that respect.

The author had also referred in his paper to the waste of oil during its transport, by which apparently he was under the impression that the oil was lost in the transport by leakage or some other form. As a matter of fact, the waste that took place in transport was merely the waste of evaporation, which he did not think any chemist in the world would ever be able to overcome. Instead of that waste being 1 %, which was the figure the author quoted, the average waste in transport between the place of production and this country was something less than ½ %.

The author also seemed to think that the discovery of toluol was due to some particular staff of chemists of one of the oil Companies. That was a mistake. Toluol was known for years before it was used in the manufacture of high explosives. It had been a matter of general knowledge that many petroleums in the world contained toluol, and the extremely valuable services which had been rendered by the Company to which the author referred were not in the direction of the discovery of toluol, but of being able to supply at once oil containing a substantial quantity of toluol, and plant with which to extract it, which no other Company was in a position to do at the moment.

He also desired to correct the figure the author had given in regard to the relative efficiency of oil to coal under boilers, being in the ratio of 3 to 2. These figures represented only the thermal comparison; the actual comparison, taking into account all the advantages and disadvantages mentioned, would be at least 2 to 1; and for warship purposes probably more. This point was so important that he thought it ought to be mentioned.

Dr. Dunstan said he had obtained the impression from the paper that the author thought that the isolation of toluol from petroleum came into being through the activities of some body of continental experts. To his knowledge the actual discoverer of toluol in

Borneo petroleum was a British chemist, the late Mr. H. O. Jones of Cambridge, who in a paper published 12 years ago described how he isolated that particular material from Borneo benzine; and it was probably a matter of common knowledge that a large amount of work was done in this country on a variety of petroleums on the same lines, resulting in the actual technical isolation of the substance in the form of a high explosive.

In connection with the author's statements as to the need for research, he had probably come across a very inspiring American text-book written by Bacon and Hamor on the American industry, in which a whole chapter was devoted to the potentialities of petroleum as a source of synthetic manufacture. For example, the possibility was indicated in the very near future of the synthesis from petroleum of glycerol and of fatty acids like stearic acid, and thereby the building up of actual food substances.

Mr. E. H. C. Craig said he desired to refer to two of the many subjects which the author had raised in the course of his most interesting paper. In the first place, he desired to say how delightful it was to hear an absolutely unbiassed and independent observer speaking with authority on many subjects in connection with which many people had been "crying in the wilderness" for years past. Petroleum education had hardly had a fair chance in this country yet, so that it was rather unfair to say that it had not been successful. It had not been successful chiefly because between academic work and the very practical work of finding and dealing with oil, there was a gulf which it was difficult to bridge, and the academic teachers and professors in this country had, he was afraid, a very amateurish knowledge of the very practical subject of petroleum. He knew, however, that an attempt was being made to make the teaching more and more practical. At South Kensington, for instance, the teaching staff had insisted upon actual field mapping for geologists being done. They made the students go out into the field for fortnights, and map ground they had never seen before, and he believed it was the intention not to give a diploma in petroleum to anyone who had not actually done some engineering work in drilling.

The only other point to which he wished to refer was the work of the Petroleum Research Department. Since the lamented death of their great chief, Sir Boverton Redwood, it devolved upon him to speak for that Department. The policy of that Department was the production of oil, or to give advice about the production of oil, from every known source in the country, including drilling for oil, the utilisation of cannel coals and allied minerals and seams, the utilisation of the Norfolk and Dorset shales, of lignite deposits

and of peat. A Progress Report had been issued by that Department, and had been asked for once or twice in the House of Commons, but the answer given in the House was always that it was not in the public interest that the Report should be laid upon the table. Some very funny things happened during the war. There was a Research Department which gave advice, which he was afraid was pigeonholed and buried. There was a Mineral Oil Production Department which did not produce oil, but which did some research work, with, perhaps, not very fortunate results. It produced friction: if there had been a little more oil, probably there would have been much less friction! Then there was a Fuel Research Board which produced Reports but not research, but this Board was very anxious not to "poach upon the preserves of established industries." Then there was a Committee of Enquiry, which rather condemned a Department which had carried out its instructions, and approved of one that had apparently made no attempt to carry them out; in fact, he was afraid that nearly all the cries of the Northcliffe press of "Too late," "Dilly and Dally," "Wait and see" and "Mess and muddle" were more or less applicable to the handling of oil-matters in this country during the war. He could say a great deal more on the subject, but he believed there was still some form of Defence of the Realm Act existing, and perhaps he had said quite enough already.

A good deal had been heard lately about the drilling for oil in Derbyshire. It had perhaps been done on rather a lavish scale—one might almost say on a reckless scale, and the result, if he might express it in the emendation of a Latin tag, was "*parturiunt montes, nascitur ridiculum petroliculum.*"

He hoped the author's clarion call that they should at any rate be careful of waste of oil, money or brains (even if the waste was not quite so bad as he suggested, and even if the Navy was not so terribly bad as some people seemed to think the author had suggested) would go home not only to everyone present, but to the officials, Members of Parliament, and the people who ruled over them generally.

The vote of thanks was then put and carried with acclamation.

Admiral Dumas, in reply, said that when anybody who did not know much about a subject in detail read a paper before a lot of experts, he must expect to be laid out in all directions; and he could not tell the members how much he was obliged to them for not laying him out more. He thanked the members sincerely for the kind manner with which he had been dealt. One of the speakers had said that he did not believe that he (Admiral Dumas)

was serious in what he had written. He could assure him that the contrary was the case. He believed there was a great waste of oil, which might be stopped. Sir George Goodwin had said that it was impossible at present to replace steam turbines by internal-combustion engines, and Sir William Pakenham had said that he did not give sufficient credit to the cleverness of the Admiralty. It was because he could give sufficient credit to them, and because he knew Sir George Goodwin was one of the cleverest men he had ever met, that he had made the remarks he had done in his paper, for Sir George could and would devise the internal-combustion engine that was required.

He was delighted to hear what had been said in regard to toluol, the abundance of which, obtained from a special oil about which they knew very little, had played such an important part in the production of high explosives.

Lastly, he was gratified to hear that there was an army of chemists at work dealing with all the scientific aspects of oil.

The meeting then terminated.

Mr. G. Howell subsequently wrote as follows :—

No one listened with more interest than I to Admiral Dumas' " volatile " spirited call to the Petroleum profession to " wake up." But is it not well for us all to remember that it was only in 1854, in Pennsylvania, that the first well was sunk by Colonel Drake. The development of the petroleum industry is, comparatively, recent, an industry which soon disclosed an essential but valuable power, from the fact that crude oil contained a great number and variety of products, a variety so great that one engineer said that practically every degree of boiling-point contained a separate product. From 1857 the industry marched slowly. It was not until 1879 that Mr. Ragozine, the founder of a method of manufacturing oil for lubricating purposes, put down his first distillation plant, to deal with 70 % of the crude oil, in the form of residues, which was being burned to waste in the fields. Blacktown, Baku, got its name from the " blacking " process it got from the primitive methods adopted in Baku in the middle of the nineteenth century when oil-men were trying to refine crude oil.

In oil progress a great deal of superstition had to be got over, also fear of using oil was a great drawback. The value of oil fuel for boilers was no easy matter to demonstrate. A large cotton-printing factory in Moscow was the first to use this oil in large quantities. But the proprietor was very chary when the first experiments were to be made. He insisted that the boiler should be erected at least half-a-mile away, in case of accident. In 1914,

as a result of this trial, Moscow was consuming over 2,000,000 tons of liquid fuel every year.

Next, take the middle product, solar oil, an oil that was practically of no use a decade or two ago. It was extremely difficult to get even those large Gasworks, the Gas Light and Coke Company, who were using cannel coal for gas-enrichment, to experiment with solar oil. In 1913 no less than 70,000,000 gallons of this " useless " oil was imported into the United Kingdom for gas-enrichment.

Again, let us take motor spirit which to-day is not only valuable but sought after as a kind of luxury. At one time this product was allowed to burn to waste. To-day considerably more than 100,000,000 gallons are consumed in the United Kingdom alone.

Here are three great " oil values " conserved from waste, values, which thanks to the profession, saved us from defeat in the late war.

REVIEWS.

A Treatise on British Mineral Oil. Editor, J. Arthur Greene. Pp. xi, 233. 8vo, *London* (Charles Griffin & Co., Ltd.), 1919.

The names of the several contributors to, and of the editor of, this important contribution to the literature of Mineral Oils are a sufficient guarantee of the thoroughness with which each division of the work is handled.

It has been pointed out by speakers and writers, as published in our previous volumes and elsewhere, that the term petroleum, rock-oil, is appropriately applicable to every description of oil of mineral origin, either natural or artificially produced. Whether in the latter case the process adopted is simple distillation, the more volatile compounds separating from the less so, metamorphosis by the " cracking " of complex molecules into simpler, or synthesis by the union of the simpler into the more complex, all the resulting oils, paraffins, olefines, aromatics or whatever they may be, are rightly termed petroleum, as being oils of mineral origin. We therefore welcome the work in hand as a valuable addition to the literature of petroleum.

The volume opens with a " foreword " by Sir Boverton Redwood, followed by Mr. E. H. Cunningham Craig on the geological occurrence of oil-shales, lignite, cannel and other coals, black-band ironstone and peat, from all of which may be extracted artificially the petroleum which Nature has failed to furnish ready-made within the British Isles, at least in quantity of commercial value.

The initial step in the manufacture, retorting, is handled by Drs. W. R. Ormandy and F. M. Perkin, and the refining by Mr.

Andrew Campbell. The chemical aspects of the subject are dealt with by Dr. A. E. Dunstan, and the utilisation of the products in the evolution of power by Mr. A. Hugh Seabrook.

As the editorial introduction explains, the book is the fruit of the collaboration of several practical scientists, each a specialist in his own branch of the subject, and its aim is to sum up and present the chief results of the latest researches in such a manner as to render available the information that has been acquired bearing upon the one practical point—the establishment of a new industry of national importance. It does not claim to be the last, but is the latest, word upon the subject of retortable material and its commercial treatment. In brief form it brings the subject up to date from several points of view, and goes some way towards the solution of a number of difficult problems. It has been a labour of love by several very busy men, who from motives both of patriotism and the keen interest of the scientist have done their best to develop a new industry, with the hope of advancing the knowledge of the day and helping those who come after.

Of necessity, no very large element of originality pervades a work of this nature: long-known and recently discovered facts have to be marshalled in their respective relations, and the acumen of the several contributors has to be concentrated on discrimination of the relative importance of the evidence selected in the judicial summaries presented to the reader.

The Condensed Chemical Dictionary. A reference volume for all requiring quick access to a large amount of essential data regarding chemicals, and other substances used in manufacturing and laboratory work. Compiled and edited by the Editorial Staff of the Chemical Engineering Catalog. Pp. 525. 8vo, *New York*, 1919.

The comprehensive title sufficiently indicates the scope of this useful compilation, effected under the editorship of Mr. F. M. Turner, Jun., with the assistance of Messrs. D. D. Berolzheimer, W. P. Cutter and J. Helfrich.

A variety of particulars is given under each heading (except the copious cross-references necessitated by commercial nomenclature), conveying the information most likely to be required in respect of each substance by persons not well acquainted therewith: in short, like all dictionaries, the work is compiled to supply knowledge to those who need it.

To the dictionary proper are appended useful tables of atomic weights, common and metric measures, temperatures and specific gravities, and transport regulations for dangerous goods.